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AMERICAN ANTIQUITY

VOL. X APRIL, 1945 No. 4

AN INTERPRETATION OF SIOUAN ARCHAEOLOGY IN THE PIEDMONT OF NORTH CAROLINA AND VIRGINIA

JAMES B. GRIFFIN

THIS PAPER is a brief statement of my interpretation of the cultural relationships of Siouan archaeological material recently excavated in North Carolina. It was written in December, 1940. Some of the pottery is now in the Ceramic Repository, but the great majority is in the Laboratory of Anthropology and Archaeology at the University of North Carolina. A short visit to Chapel Hill during the first week in November, 1940, enabled me to obtain a bird's-eye view of North Carolina archaeology. This was possible because of Mr. Coe's familiarity with the area and because there is sufficient material at Chapel Hill to provide a sample of the diverse archaeological units found in the state. While Mr. Coe supplied me with much of the information contained herein, he is not responsible for any errors of commission or sins of omission.

A re-examination of the historic locations and movements of the Siouan tribes of the Piedmont may provide a firmer background for the understanding and interpretation of the archaeological material found on the sites occupied by these tribes. Since it is impossible at this time to make a critical examination of the primary sources of this historical information, it is necessary to rely on the available secondary sources. A division into southern and northern Piedmont Siouan groups has been accepted by Speck and Swanton. From the southern "of two very distinct dialectic groups,"[1] in which have been placed the Cheraw, Keyauwee, Eno, Catawba, Waxhaw, Shakori, Santee, Winyaw, Sewee, and Waccamaw, there are available vocabularies of one tribe only—the Catawba. Of the northern group of Monacan, Manahoac, Saponi, Tutelo, Occaneechi, Nahyssan, and Mohetan, we have the vocabulary from Tutelo alone. Because of this, it would be well to speak of the "Virginia" Siouans as the Tutelo division and of the "Carolina" Siouans as the Catawba division. No one has yet given any very accurate idea

[1] Swanton, 1936, p. 379.

of the degree of divergence of these two linguistic units or the meaning, historically, of this divergence. However, the recognized local bands or tribes of the Catawba division were differentiated by 1520–1540, those of the northern, or Tutelo, division, by 1600.

It has been suggested that the Siouan tribes of the Catawba division found in North Carolina by Lederer and Lawson had moved in shortly before 1670. It was Swanton's opinion in 1936 that this movement of some of the Catawba Siouans (Sara and Keyauwee) into North Carolina was the result, partly of Pardo's journeys into the interior in 1566, and partly of an assumed movement of the Cherokee Nation from some point on the upper Ohio, up the Kanawha and New rivers to the headwaters, and then down the Clinch and Holston as far as Chattanooga.[2] This Cherokee movement took place before the arrival of the Spaniards —hence at some time before 1540. The Cherokee found Muskhogean people to the south of them and Siouan tribes of the Catawba division to the east of them. According to this interpretation, the movement of the Catawba out of their postulated home in the Ohio Valley probably took place not later than about 1450.

The story of the historic movements of the Tutelo division is somewhat different. The Manahoac and Monacan units were already located along the Rappahannock-Rapidan and James valleys, between the fall-line and the mountains, in about 1600. We do not hear of the Tutelo-Occaneechi-Saponi as such until some time later, namely around 1670, when they were south of the territory occupied by the Manahoac and Monacan at the beginning of the seventeenth century. Bushnell has called some of the Monacan groups Tutelo and Saponi.[3] Swanton has suggested that the location of a village called Mohetan, which may or may not have been Siouan, in the lower Kanawha Valley in 1674 indicates a movement of the Mohetan tribe from the Ohio Valley into Virginia.[4] It is known, however, that this tribe was on the eastern side of the mountains in 1669–1670 and hence was going westward. In any event, an east to west movement in 1670–1673 can hardly be used as evidence for the location in Virginia in 1600 of tribes that came from the Ohio Valley. Nor can an erroneous location of the Tutelo on the Big Sandy in 1699 be used as historical evidence for an Ohio Valley home of the Tutelo.[5]

A different view of the location of the pre-1600 homes of the Tutelo division is suggested by Speck, who feels that the Powhatan and other Algonquian tribes of the Tidewater area displaced the Siouan groups, forcing them into the Piedmont section some 200 years or more before the coming of the Europeans.[6] If this hunch were correct, any move-

[2] Swanton, 1936, p. 379. [3] Bushnell, 1930. [4] Swanton, 1936, p. 373.
[5] Griffin, 1942. [6] Speck, 1935, p. 203.

ment of the Tutelo division out of the Ohio Valley would necessarily have occurred some time before 1400.

In any event, the historical evidence suggests that the late seventeenth- and early eighteenth- century merging of the Tutelo and Catawba groups in north-central North Carolina and south-central Virginia was the result of pressure on the Catawba division from the south and east and of pressure on the Tutelo division from the north and east.

SIOUAN ARCHAEOLOGICAL MATERIAL FROM THE CAROLINA AREA

Mr. Coe believes he has good archaeological evidence from the vicinity of Clarksville, Mecklinburg County, Virginia, to substantiate the historical evidence that three islands in the Roanoke River were occupied by the Tutelo, Occoneechi, and Saponi tribes of the Siouan linguistic stock. There are indications of at least three archaeological horizons in this locality, and it would of course be a methodological error to confuse the material from these three time levels and to attribute all archaeological remains in the area to the Siouan group. According to Coe, pottery of the oldest type is found in the sand which formed the various islands and may well have been deposited by stream action. The pottery is reddish in color and grit-tempered, and its surface has been malleated by a cord-wrapped paddle. The second archaeological horizon may be identified by pottery with a fabric-impressed surface and with a texture coarser than that of the later pottery. The best-known site of this period is on the south mainland opposite the middle of Tutelo Island. Rims of sherds from this site vary between straight and inward sloping, they are usually unmodified and rarely have an added rim strip. The lip is usually rounded and of the same thickness as the wall.

The third archaeological horizon is identified with the Siouan occupation: the Tutelo on the first island, the Occoneechi on the second, and the Saponi on the third. On the first island, the aboriginal material is associated with European trade objects, as it is on a fourth island which can not be historically identified with a tribal group. The aboriginal material from these sites is homogeneous and is closely related culturally with that from Lower and Upper Sara town in North Carolina. On the north mainland opposite Occoneechi Island, where the Occoneechi had their corn fields, there is an historic site which has produced scissors, a seal, and a six-inch copper disk. A skull from the site is in the Arthur Robinson collection in Chase City.

These sites of the Tutelo division in south-central Virginia are of the 1650–1675 period; comparable sites of the Catawba division in North Carolina are Upper and Lower Sara towns in Stokes and Rockingham counties, respectively.

Saponi Town. The Saponi town of approximately 1700 is located on

the north side of the Yadkin, near Cotton Cove, in Davidson County, North Carolina. The exterior surface of pottery from this site in the Ceramic Repository is almost always net-impressed. Approximately one-third of the rims have an added rim strip on the exterior or have the rim folded onto the outside to produce the same effect; the strip or fold is covered with the fabric impression. The interior surfaces are well smoothed and are uniformly black to dark gray. The paste has coarse grit temper with rather sparse quantities of crushed rock. The outer edges of the lip of one of the sherds with a rim band has transverse gashes spaced a centimeter apart.

Coe believes that some low mounds at the site do not belong to the later Siouan complex. It is not known that the Piedmont Siouan tribes in North Carolina and southern Virginia built mounds, and, as far as I know, the same thing also holds true for the Catawba. Coe believes that the pottery from this site is a cross between the material which has been described for Lower Sara town and the pottery found at such sites as the Franklin Mound in Macon County, North Carolina. This latter site shows a mixture of concepts found at the sites Harrington called Cherokee and concepts found at sites of the Lamar period in Georgia. The fundamental characteristics of the Saponi site indicate a distinct resemblance to Keyauwee, but different percentages of types of surface finish and other features will be obtained. The pottery has smoothed surfaces; simple stamping; a crude, large, complicated stamp related to Lamar Complicated Stamp; and occasional examples of annular punctates in a horizontal row around the outer rim just below the lip. This latter idea apparently came into the Saponi complex from sites farther down the Yadkin-Pedee drainage. The added or folded rim strip is not particularly common at this site. The rim shapes indicate a gradually flaring rim which is probably placed on a round-bottomed, wide-mouthed jar.

Occaneechi. The Occaneechi site of about 1700 is located about a half mile east of Hillsboro on Eno River, in Orange County, North Carolina. Prior to their occupation of this site the Occaneechi had been on the middle and largest of the islands in the Roanoke River south of Clarksville, Virginia. They were getting ready to leave the site when they were visited by John Lawson in 1701; hence, since they were still in Virginia in 1675, they could not have lived here more than twenty-five years, and probably less than that.

Coe began excavation on this site with a trench in 1938, and further excavations were in progress at the time of writing. The houses are circular, with no indication of center posts, but with a multiple outer wall. They are 22 to 25 feet in diameter. The fireplace is a shallow pit, not a prepared circular or square structure. Fire-cracked stones were found in shallow pits within as well as without the house. A single flexed

burial placed on the left side and accompanied by a pipe and pot was found in the center of a house.

Occasional trade objects are found, but their small number helps to indicate the relative age of the site. Polished stone objects are rare. The most common points are of silicified slate and are triangular in shape. One of these points has serrated sides. A somewhat smaller number of stemmed points are slightly larger than the triangular ones. The knives are stemmed. The awls are mostly of the split-bone type. There is a small number of turkey metatarsal awls, some of which have the notched ridge characteristic of the majority of the Fort Ancient sites. This trait also occurs very rarely in sites in eastern Tennessee thought by some to have been occupied by protohistoric Cherokee. One beamer of deer metatarsal has been found, and there are also a few antler flakers. So far, bone implements or ornaments are not particularly common.

Shell is not commonly used for implements. Serrations on the edge of a number of small valves were produced by closely spaced transverse notches. Marginella beads and a very small disk beads complete the list of shell ornaments.

The pottery is grit-tempered; it has a rather sandy appearance and larger white angular particles have been sparsely used. Mica flakes are visible in some of the sherds. The texture is medium-fine to medium. The most common surface finish is simple stamping with a crude check stamp; cord-marked, net-impressed, and plain sherds and sherds with impressions of a peculiar type of plain plaiting which looks like check stamp are in the minority.

There are a number of vessel shapes. Some are conical with sides gradually sloping out, some with flaring rims and subconical to rounded bases are roughly cylindrical; and some with deep subconical bases have rims curving in. Approximately one-third to one-half of the rims of jars have an added outer rim strip with notches or punches just below the lip or placed transversely on the lip surface. This treatment of the rim is associated with jars having flaring rims. Incurving rims sometimes have an incised design suggestive of Lamar Bold Incised; the surface of these vessels is usually smoothed.

Lower Sara Town. Lower Sara town is thought to be located on the Dan River, about two miles southeast of Leaksville, in Rockingham County. The pottery in general belongs among Holmes' Middle Atlantic wares and would now be considered a subdivision of Woodland pottery. The paste has a rather sandy appearance and feel which is probably due to the nature of the clay used in its construction. Angular fragments of crushed rock, small to medium in size, appear sparingly. A noticeable amount of small mica fragments in the clay reflects the light. Colors are a reddish to gray buff on the exterior, while the interior

is most often dark gray to black. The color of the core varies considerably according to depth of penetration of oxidation from the exterior surface.

Exterior surfaces may be variously finished. A small proportion are cord-marked, and an incised design is superimposed on some of the rims which have been partially smoothed. Other surfaces have been combed or brushed. The majority of the sherds from this site in the Ceramic Repository bear impressions of a net, the knots of which are approximately one centimeter apart. Three sherds have impressions of a much finer net, with knots spaced only about four millimeters apart. This is a less common type. An examination of the collection in Chapel Hill indicated that net impressions were relatively rare as a surface feature of pottery. Also rare are sherds decorated with a complicated curvilinear stamp probably correlated with Lamar and Savannah Complicated Stamp, the style found at Keyauwee. One subconical jar has rather marked shoulders and a slightly flaring rim; on the exterior surface of the body is the impression of a coarse net like those Holmes figures from North Carolina.[7] Almost constant features of the inner surfaces of these sherds are roughly horizontal striations caused by the scraping which produced the desired thickness of the walls. Some resemble scratches, while others are shallow grooves.

Supplementary decoration is relatively sparse. Lips are rarely transversely notched or impressed but are more often notched on the outer edge of the lip. A few specimens have a horizontal row of fingernail punctates along the base of a slightly flared rim. One crude effigy figure faces inward on a bowl rim. The lower edge of one small strap-like handle which was placed on a low, almost vertical, rim is riveted into the shoulder. On the handle are three rows of horizontally placed vertical punctates.

The rims are thinned as they approach the lip and are vertical to slightly flaring. A folded or added rim strip is rare.

Keyauwee. The Keyauwee village site was also occupied at about 1700 and it too was visited by John Lawson. It is located some seven miles west of Asheboro, on Carraway Creek, in Randolph County, North Carolina. Swanton has classified these people with his southern Siouan or Carolina division, but, as has been pointed out, all that is really known about them is that Lawson visited them. Coe has already presented a number of brief statements on the excavations at this site.[8]

During the winter of 1937–1938, Joffre Coe submitted the pottery from the Keyauwee site to the Ceramic Repository for analysis. Characteristics of paste are similar to those of the general run of South Ap-

[7] Holmes, 1903, Pl. 129, lower center, and Fig. 33. [8] Coe, 1936, 1937.

palachian and Middle Atlantic types; it has a slightly sandy feel and includes rather sparse, angular particles of grit as tempering material. Among 2273 sherds, the most significant single feature, surface finish and decoration, was divided as follows:

Smoothed	34.4%
Combed-brushed	15.4
Fabric-Net	16.0
Cord-wrapped paddle	5.0
Incised	3.3
Check Stamp	3.8
Curvilinear Stamp	2.1
Doubtful	20.0

A large proportion of arrow points is triangular, with some variation in shape of sides and base. Some could be regarded as "diagnostic" of the Mississippi pattern, but the total cultural complex at Keyauwee is distinctly not Mississippian. Although the most common shape for pipes is similar to that of English and Dutch trade pipes, it is also the shape associated with Carolina Siouan sites. Pipes are made of local clays. Burials were fully flexed and placed on the side. Finds include disk beads of shell, beads of bird bone, a pendant made from the scapula of a small mammal, discoidal stones, Marginella beads, a small copper-covered ear ornament, an alate-stemmed platform pipe, historic glass beads, and stems of trade pipes.

Available evidence from the sites attributable to the Siouan tribes of North Carolina, 1650–1700, indicates a relatively homogeneous archaeological culture which could possibly be placed in a division of about the same magnitude as the aspect, in the McKern classification. It is also evident that the material, which is completely at home in this particular area, does not require the postulation of a recent immigration of the Siouan group. Ceramics from northern sites reflect proximity to the Chesapeake-Potomac area during the first part of the period under discussion, while those from later sites, inhabited by the same tribes after their movement south, reflect their position closer to the complicated-stamp area and to tribes coming northward. Pottery made by these northward-moving people bore complicated stamped decoration of the late protohistoric and historic periods. The pottery of these Siouan tribes can be placed without hesitation among Woodland wares. On the basis of archaeological evidence throughout the Southeast, we know that there was a long period during which Woodland pottery was common in the South Appalachian and Piedmont-Coastal area. No archaeological evidence is available to indicate a movement of Siouan peoples into the area.

A small body of comparative data is available as result of David I.

Bushnell's work on the identification of sites occupied during the early historic period by Virginian Siouan tribes. He has also collected surface material from the sites. His work has not been as extensive as that of Coe in North Carolina, a fact partially accounted for by the earlier dates of the Manahoac and Monacan occupation which he describes. As Bushnell points out, while the Manahoac group occupied the valleys of the Rappahannock and Rapidan west of Fredericksburg, Virginia, between about 1600 and about 1650, it is not possible at present to locate satisfactorily any one of their villages.[9] Somewhat more assurance is voiced by Bushnell regarding roughly contemporary villages in the James Valley possibly occupied by Monacan tribes[10] who became known as the Tutelo-Occonee-Saponi, at the time of Lederer and Lawson. They form one of the divisions of Swanton's Virginia Siouan group,[11] the others being Manahoac.

Bushnell illustrates pottery from only one site, Monasukapanough, attributed to the Saponi, and located a short distance north of the University of Virginia, near Charlottesville. Two of four rim sherds apparently had a folded or added outer rim strip with a horizontal row of large, deep, oval punctates at its base. The pottery is fabric-impressed, cord-marked,[12] grit-tempered, and conforms in general appearance to Chesapeake-Potomac ware, or Woodland. It is not particularly close to the Siouan material of Coe's sites, but the two are considerably closer to each other than either is to any assemblage of Fort Ancient pottery.

During the period 1891–1893, Gerard Fowke made a survey of the Piedmont area of West Virginia, Virginia, and Maryland, with the avowed purpose of connecting remains along the Coastal Plain with those from the Ohio Valley. In the valley of the James, he excavated a large village site at Gala in Botetourt County, Virginia.[13] Abundant material was found in a non-acid soil in which artifacts remained in good condition. The preservation of a large number of artifacts of bone and shell helped to give the impression that the site belonged to the Fort Ancient culture. Material from here was exhibited in the U. S. National Museum in 1936 as from a Fort Ancient site. A comparative study, however, would indicate a cultural disparity of a rather high order, although some evidence suggests that the site was approximately contemporaneous with the Fort Ancient aspect.

CONCLUSIONS

The study of cultural remains from sites attributed to eastern Siouan tribes was undertaken in order to obtain an idea of their cultural homo-

[9] Bushnell, 1935. [10] Bushnell, 1930. [11] Swanton, 1936, p. 374.
[12] Bushnell, 1930, p. 11. [13] Fowke, 1894, pp. 17–23.

geneity and to indicate their cultural position with regard to artifacts attributed to neighboring contemporaneous groups and to the prehistoric cultures of the same and adjoining areas. The main cultural affiliations of the northern division of the Siouan tribes are with the complex of artifacts from the coastal region known to belong to the Algonquian tribes of the Powhatan confederacy and related groups. Swanton has suggested that the Keyauwee belong to the southern Siouan division and that there was a fundamental linguistic and presumably cultural difference between the Catawba tribal assemblage and the northern group. While there is sufficient cultural evidence only from the Keyauwee unit attributed to the southern group, we are not justified archaeologically in making such a statement. While it is true that material from the Keyauwee of 1701 shows more of the complicated and check-stamped ware of the southern area than does that from northern sites of 1600–1675, so does that of the Saponi town of approximately 1700.

Sites of some of the southern group of Siouan tribes, such as the Catawba, Waxhaw, Cheraw and others, must be identified in order to determine whether there is a fundamental archaeological difference between them and sites of the northern division.

Archaeological material now attributed to the Siouan tribes of the Piedmont could have been derived as well from antecedent cultural stages in that area as from any other area.

The point of view presented in this paper is that the Fort Ancient culture could not represent the village sites of the ancestors of the Tutelo-Catawba groups, because, according to the available archaeological evidence, there is a wide divergence between the Fort Ancient culture and that attributable to the eastern Siouan tribes, and also because the Siouan tribes are known to have occupied eastern sites at a time approximately contemporaneous with the probable dates of occupation of Fort Ancient sites.

The archaeological division in the Ohio Valley which preceded the Fort Ancient culture is the Hopewell division, of which the Adena complex is either a ceremonial cult, or a preceding stage. Hence, if the hypothesis that the Siouan tribes of the east formerly occupied the Ohio Valley were valid they should have possessed a material culture classifiable under the general division of Adena-Hopewell or belonging to a still earlier period. Is there, then, material in the Piedmont area of Virginia and the Carolinas and in the intervening area which would indicate a cultural connection with the Hopewellian phase, and, if so, what is the explanation of that cultural connection? While there is relatively little evidence, both quantitatively and qualitatively, for this horizon in this area, there is enough to indicate some degree of cultural connection. Mound groups in the Kanawha valley around Charleston,

West Virginia, excavated by the Bureau of American Ethnology about 1870–1880,[14] gave evidence of Adena occupation, and recently excavated sites in the Big Sandy Valley in Kentucky show the same general type of material.[15] The presence of predominantly stemmed and notched points, bannerstones, expanded-center bar gorgets, and pottery of Woodland type known to belong in the Hopewell period in a number of known sites in North Carolina, such as the Puette site in Transylvania County and the Hardaway site in Stanley County, strongly suggests that they belong in the Hopewellian period. These sites need excavation. At present, they look as though they were contemporary with, and not derived from, Ohio Valley Hopewellian sites.

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August, 1943

[14] Thomas, 1894. [15] Webb and Haag, personal communication.

THE WRITING OF AMERICAN ARCHAEOLOGY

MARGARET W. HARRISON

AT A LITERARY tea given by Robert Nathan's fictional firm of Laocoön Ltd., the company's imprint was thought by some to represent publishers struggling with a group of authors in the form of serpents. After a decade's editing of archaeological manuscripts, I have learned that this is what editors or publishers might think, but I also know how authors would interpret it. Thought of a blue pencil rises to apprehension as they submit a brainchild to the far from indulgent eye of a publisher-stepparent. But no manuscript is ever perfect, and by the constructive criticism of troublesome editors it is rescued from slips that escape the busy archaeologist. He may therefore find in an editorial stocktaking a useful consideration of the present state and possible improvement of archaeological writing.

In the annals of American archaeology lie the records of achievement wherewith to trace the remote heritages of our western civilization. Yet these accounts often repel both student and general reader by the quality of expression, which beclouds with dullness and diffusion material of high value. That archaeological writing need not be unattractive I am convinced, despite much proof of indifference to the value of style in communicating substance, for good writing is but the technique of conveying to others the fruits of research that archaeology seeks to add to the sum of knowledge.

A double task of analysis and synthesis faces everyone who writes about the past. Accumulation of materials, engrossing as that may be, is for the student in historical discipline only the first step in constructing the outline of early cultures. After collecting and sorting the evidence, the archaeologist is under obligation to use and shape it. Though a report may claim to be no more than a catalogue of facts, even here lies a clear distinction between good and bad arrangement or statement; but when the archaeologist has compiled the facts and advanced to interpretation of them he becomes a writer no less than an investigator. Because he labors to be useful, it behooves him not only to employ the critical method of science in searching for materials of archaeology but just as fully to express his evaluation of results as clearly and forcibly as he can. These results may not be conclusive, but they should be synthesized as far as experience will allow.

Perusal of the average archaeological report causes the reader to reflect that, though the information had value, only his own perseverance outlasted the tedium of vague, halting sentences. This is hardly a fair or courteous burden to place upon a reader who, likely to be a fellow

student, is himself pressed for time that the author of the paper neglected to take in its preparation. In ill-written lines, moreover, the author risks injustice to himself, for failure to clarify and order material may represent failure to clarify ideas.

The aim of writing, particularly of scientific exposition or historical narrative, is to communicate information and ideas. The archaeologist may not assume the modern poet's right of using language to release emotion without being expected to make it understandable to any but himself; for, if an investigator's results are to achieve currency in the body of scientific fact and theory, they require description in words that are intelligible and interesting. Formless, colorless prose impresses no one. Many a field report contains excellent but unworked material; and, though stuff is better than style, stone and rubble are not a pyramid.

As the archaeologist seldom lacks the prime requisite of good writing —a definite purpose—his first objective is clarity. Slovenly reporting is comparable to superficial excavation; to inexact notes on position, measurement, and direction; to careless and incomplete references. Slipshod writing tends to cover up slipshod thinking; obscure writing, to emanate from confused and unripe thinking. It is dangerously easy, by disorderly and hazy expression of details, to slur over inaccuracies and omissions. Present-day archaeologists may take heart, nevertheless, from Darwin, one of the most lucid of scientific writers, who remembered that difficulty in expressing himself clearly and concisely had the advantage of forcing him to think intently about every sentence and thus to perceive errors in his reasoning and observations. If too readily satisfied with a cloudy text, the author imposes the uncertainty of guesswork upon the reader, who needs to see plainly what is before him, without interpretive overlays from his own fund of understanding.

With clarity and order secure, interest is the next consideration. Some maintain that archaeological exposition has no concern with interest or attractiveness, but those who take this austere view may not then complain if what they write does not find readers. Prose that is correct but harsh and ugly becomes tedious; attention wanders and lags. That scientific discourse proceed lucidly is, indeed, its first obligation, but a pleasingly written paper gains influence, whereas angular, unrhythmic composition has no appeal—and no audience. A good prose style is essentially clearness and movement, the faculty not only of making plain whatever is said but of keeping alive the ideas that have been created.

Good writing is difficult. It demands patience and effort. No one complains because the ability to take good photographs, identify potsherds, or determine building strata requires practice and study, yet the archaeologist is apt to look upon writing as a mere knack, endowed

by nature. Irked by this unfamiliar mode of expression—unfamiliar because time for its cultivation is scarce—he takes refuge in thinking that techniques of preparing a paper are unimportant, in believing that anyhow will do. "We worked at intervals over several years" or "So-and-so was late in finishing his part and I had no time to check it" are common excuses for the unorganized report that so often comes from the field worker. Or, lured by the will-o'-the-wisp of "one more sherd," he pries endlessly into heaps of debris and never emerges to do more than list the finds. For most of us composition is painful, but the archaeologist, careful to observe accuracy and thoroughness in collecting data, should be especially aware that form and matter cannot be dissociated, that, if he writes clumsily and disjointedly, his thoughts are undisciplined. To weigh words is to weigh thought, and much depends upon the choice of language that describes theories and their supporting evidence.

Once convinced that if material is worth publishing it must be communicated effectively, the investigator is confronted by the specific act of writing: planning, writing, revising. If certain of what is to be said and to whom, he is ready to lay out the scheme of the report. Not a few papers seemingly skip this stage in the procedure, plunge directly into the second, and ignore the last, all in the innocent belief that spontaneous expression naturally bubbles into well-ordered prose. But, because composition is a progression, it has a beginning, a middle, and an end. There is no successful escape, therefore, from deciding what will be placed in the introduction, in the development, or in the conclusion.

An introduction exists to put the subject before the reader. It should have definite connection with the subject and should promise only what will be fulfilled in the text. At this point archaeological reports use to advantage a statement not only of the problem but of the main results and their general significance, so that the reader will have a background against which to judge the details that follow, instead of being thrown into a mass of unrelated minutiae. From the outset the reader wants to see clearly the direction of the discourse, for, unless prepared for the general theme, he must retrace his steps time upon time to appreciate the proper place and value of each phase of the exposition.

Now introduced, the thesis is developed in the text. The data, depending on their nature, may be arranged by chronological order or by juxtaposition of sites or areas examined. Insurance against straying from either plan means writing from a well-organized outline that divides the subject into sections of parallel weight, gives first place to main topics and holds subsidiary matter subordinate, meticulously weeding out every upstart item.

Archaeological events and processes arrive at a logical ending,

whereby natural conclusions or discussions appear from the antecedent facts. Whether it be published or not, a summary offers an excellent test of order, for, if the author cannot list the main points, the reader also may miss them, a sign that the structure is poor. A summary is particularly useful when the conclusions have trickled out all along the way until nothing remains but stale repetition.

An archaeological report may be a record of facts or an interpretation of them. More desirable, however, is the paper which combines both exposition and argument, for the excavator is the person best able to evaluate what he digs. But, if the report flits between fact and conjecture, it forfeits the virtues of both forms and bewilders the reader by a jumble of ill-sorted ideas. Each kind of presentation is more successful if definite and distinct. Much of archaeology must await further unearthings before results of an excavation may be safely weighed, though every shovelful tempts opinion; yet researchers in prehistory are in need of a sense of history, to remind them that events are preceded by causes and followed by effects. It is wise, therefore, not only to enumerate for the record the bones and pottery swept from the dust but also to progress to their interpretation in terms of the whole project, with balance and perspective to connect each piece in the maze of prehistoric patterns.

The writing of the documentary or factual record is the less complicated but not the less exacting task. Minutiae have to be sorted into categories, descriptions sifted into symbols. In this part of the report the elements of style and interest are superfluous, for it is supposed that no one approaches such a record without interest already aroused. What is expected, however, are complete and accurate data, set down in logical order and in concise wording. The subject is sometimes intricate enough without the added burden of obscure verbalism. This body of information is designed for other students, who want quick reference to evidence and comparison. They, as well as publishers, are here best served by telegraphic style, schematized headings, and technical terminology. The demands of material in the factual phase of the report constrain the author to make wording exact and to forego the easy phrasing of generalities. Although scientific terms, to be sure, are frequently abused and appear in statements where ordinary English is adequate and far more readable, in recording new facts we gain little by the substitution of lay descriptions for well-understood technical terms, for basically these are the scientist's shorthand. But well understood or carefully defined they must be, because archaeological descriptions are less systematized than those of other disciplines, and many terms, especially ceramic and architectural, have by no means common acceptance among archaeologists.

Still aglow with the adventure of his search, the archaeologist is inclined to recount at full length every detail uncovered, when its bearing on the immediate problem is questionable. This unwillingness to reduce new facts to their essence or to exercise even a scrupulous selectivity smacks of uncertainty in deductions and clutching at security by a display of unimpeachable technique.

By reduction to tabular form, much new material may gain in clarity for the reader—and even in economy for the publisher, despite the fact that typeset tables cost more than other composition. If the data permit an inventory of common characteristics, differences, dimensions, and so forth, a table can condense to a few lines what description would spread over a dozen. Similarly, diagrams and charts may save paragraphs of verbiage. The reader sees at a glance, without wading through countless repetitions of "average thickness is . . . . , diameters are . . . ," not only the specific measurement but the range of values. Colors, shapes, and features likewise lend themselves to such graphic presentation. As an irrefutable and efficient adjunct to discussion, however, tabulation serves to supplement, or even replace, lengthy descriptions, not to repeat what is already given *in extenso* in the text. And on the other side of the shield, tabulation warrants in the discussion only brief emphasis of the statistical highlights.

In writing the interpretive part of the report the field worker meets more difficulty. The clarity of factual recording wants coherence and emphasis to give the data meaning for the reader outside the specialized field. The admonitions of unity, coherence, and emphasis have dropped, for many nonprofessional writers, into the limbo where exercises in composition are consigned. These principles of writing are nevertheless still important to the reader, however they may annoy the author, and vague and stumbling is the essay that neglects them. So essentially simple are these precepts that by their mastery the archaeologist adds pen to trowel and brush as the tools of his trade.

Unity is only concentration and intensity: perfect clarity in the subject and restriction of every remark to a special purpose. Coherence is nothing more than order, an intelligible classification of facts. Writing should move from some point to some point, and the bearing of each detail should be unmistakable. Emphasis gives important treatment to important ideas. To further this end the archaeologist has but to dip into a wealth of illustration, verbal as well as pictorial. He also engages attention by placement of ideas at the significant positions in writing: the beginning and end of monograph, paragraph, and sentence.

In the interpretive section or essay, technical terminology tends to become gibberish. The archaeologist does well to use his "shorthand" sparingly and cautiously. A threat of Basic English might be leveled

at writers whose love for abstruse wording leads even specialists to sigh for translation. Simple, direct phrases tell more than all the windy jargon which but opens the archaeologist to the criticism that he seeks to impress others with learned sounds and to bolster flimsy structure with underpinnings of bombast. Along with simple language, there is room for allusion in interpretive writing, if the author's background in his own and related subjects be sufficiently rich. The avoidance of too early specialization and the wider cultivation of literature outside the author's first interest would add much to the attractiveness of his style. Humor and imagination are welcome, for dullness is not an indispensable characteristic of learning.

There are those who say that the archaeological report has no place for the graces of style. But fluent prose that has regard for the ear by varying length and construction of sentences, by shunning close repetition of identical or similar-sounding words, cannot be condemned as a purely stylistic facility. Philip Guedalla thinks "We are all far too much under the sway of that dangerous heresy of the late Nineteenth Century, which laid it down that if one has style, one is of necessity lacking in the deeper qualities. That is why our historians, eager to avoid all imputations of style, habitually embalm their wisdom in a dialect of the most repulsive angularity. Archaeologists demonstrate their soundness by writing like navvies; and economists, nervous of the grave discredit which might attach to a well-turned sentence, pass boldly beyond the limits of rational speech and float at ease in the outer spaces of mathematical symbolism." Archaeological reporting has far to go before its writers can be accused of phrasemongering.

Well-written accounts of excavations and their significance would win for archaeological studies a wider audience. At the present time, when knowledge has so increased that scientific advances are known more and more by only a few researchers, clear, simple, and interesting statements of such work would broaden its distribution. The importance to a liberal education of a knowledge of early civilizations needs no pleading, but dissemination of this knowledge is in straits. Much of our learning is confined to the specialist, and it will remain inaccessible to the general reader as long as the style of its writing continues to be tedious and arid, not because the subject is recondite. This is not to urge that archaeological writing take on the tone of a mystery thriller or a thundering adventure (though archaeology has elements of both), or that it strain to become literary, but to persuade those who write from pinnacles of incomprehensible erudition, who denounce interesting and attractive writing as "popular," that what is understandable to an intelligent layman is not necessarily unscientific.

To expatiate on all the literary gaucherie of which archaeological writing is susceptible cannot be encompassed here, but some faults so plague its authors that to mention them briefly may be helpful.

It is mystifying why recorders of prehistory go out of their way to use indirect construction when it but takes the life out of their sentences. The constant "there is found," "there exists," "was used," "were employed," dampen any reader's enthusiasm and abate his interest to complete passivity.

Entanglement with antecedents for relative pronouns sometimes has droll consequences. Only a contortionist could appreciate the statement that "Men cut or scorched the hair from the crown of the head but left a fringe over the forehead, which was pushed up to form a crest. . . ."

Meagerness of vocabulary in many writers whose interests center on field work renders them blunt, or unheeding, to differences in the meaning of words. A "monolith of superimposed drums" withstood a day's argument before the creator of the phrase would recognize it as a contradiction in terms. With difficulty was he persuaded to yield "malcreants" for "miscreants." Closer acquaintance with dictionary and thesaurus would prevent such mistakes and at the same time refurbish one's diction.

Both in meaning of words and in needless repetition of the same ones sparse vocabulary gives trouble. Quite apart from choosing the correct word for a description where substitution of another term would be foolish, if possible, archaeological treatises have wide latitude in their phrasing. For the ubiquitous "use," both as noun and verb, there is certainly some escape from reading it fourteen times in a page and a half. The six occurrences of "lineage" in a twelve-line paragraph show an unnecessarily narrow concentration.

Caution in placing identical words in close repetition extends to watching the proximity of similar sounds. "Chaos reigned until the rains came" beats upon the ear with all the monotony of falling weather. Laziness in picking even exact words, if imaginative ones are too much to hope for, fades an account to dun-colored fabric. In a survey of the habits of a primitive people, food was always "cooked," but from the utensils mentioned it was obvious that at least some dishes were baked or boiled. When there are at least a dozen ways to prepare food, each a distinct process, to flatten all of them to "cooking" is to lose the flavor from language.

A snare for the unwary lies in participles. They "dangle" upon the slightest relaxation of vigilance. "Scattered irregularly about cenotes one still sees the straw-thatched huts." Is the observer really dismembered—and untidily at that? Of the cannibal variety, no doubt, were

the finny ones so courted that "When starting to fish the fish were flattered by being addressed in this manner." As substantives, participles have a way of attracting objects when they are meant to attend strictly to themselves. Constructions such as "For sweetening honey was used as well as syrup . . . ," or "For weaving primitive hand-looms . . . were employed" are a double awkwardness, which could have been circuited by transposition to active voice or by insertion of a comma.

Of commas and their punctuational brethren there is no end. All that I will mention here is my belief that archaeologists would be the happier could they follow the example of "Lord" Timothy Dexter, who printed at the end of his book, *A Pickle for the Knowing Ones*, a page of assorted points so that his readers "may peper and solt as they plese."

Paragraphs, where readers stop to catch their breath, are each a separate entity. They are designed to contain some one item of the whole thesis, either as an idea in itself or as a transition from one point to the next. There are manuscripts that may be suspected of indenting automatically every few hundred words, so lacking in unity is each paragraph. The place of emphasis is at the beginning and end, for which topical and summary sentences, respectively, are convenient though not imperative. Smoothness to the whole is furnished by connectives (not, *however*, at the beginning of the sentence), which bind the logical order of statements and give the relation of each to the others. For their cadence paragraphs depend on variation in length.

To author's inertia goes the blame for a remark's lurking in a parenthesis or footnote when it is really an integral part of the discussion. If the observation is actually not pertinent, such an aside belongs nowhere in the paper; but if it is a relevant afterthought, it deserves to be worked into the text during revision.

Contrary to a notion that must prevail, to judge from the frequency of "the author" and "the writer," the moderate use of the first person singular gives no offense. Besides the confusion of thus mingling one's own opinions with those cited from other writers, the persistent evasion of "I" is an affectation. If a report is expected to give personal information and ideas, reference to oneself need be neither roundabout nor apologetic.

An author is much too busy with spade and tapemeasure to concern himself about the mechanics of publishing a report. Yet it is only sensible that he be apprised of general costs and restrictions in presenting information, so that he may decide what is of primary and what of secondary importance.

Essential to the adequate portrayal of archaeological material, illustrations tell what unaided text cannot hope to do. But, because

illustrations such as this discipline demands are the most expensive part of reproduction, the publisher has every reason to require that pictures be of first quality in subject, composition, and focus; for good photography in the field, though troublesome, is possible; and no reproduction can better the original. Archaeologists are partial to photographs that prove them meticulous technicians but show nothing of archaeological value. Handsome skeletons repose at length in mute testimony of having been taken to superior cleaners, but they further archaeology not one stone's turn. Especially cherished are photographs which show objects in situ yet are indicative of no clear stratigraphic conditions. If a reader will not accept a statement of position, he will never be convinced by a fakable photograph. It is to be remembered, finally, that every duplication, no matter how choice, usurps the place of a new illustration.

Line drawings appear in better shape because field sketches are usually turned over to a draftsman; but, if the archaeologist attempts his own illustrating, he is warned that a lack of precision in lettering may not be viewed with easy tolerance.

Without support from endowed presses, scholarship would wither on the stem, for the sale of few scientific treatises repays the cost of their printing. Yet the fact that reports of scientific research are not literary bestsellers does not mean that they are not worth publishing, as some critics urge. If their value were measured in terms of their sales, science would recede to another Dark Age.

The task of reconciling two outlooks, of presenting the archaeologist's results as faithfully and effectively as printing can do it, without losing sight of cost to the publisher who sets these vessels of scholarship afloat in the current of scientific investigation, falls to the editor. My counsel in behalf of both points of view arises from a conviction that the achievements of American archaeology recorded for students and desired by an increasing lay audience are worthy of better expression than they receive from the hands of the author, whose literary difficulties come not from any fundamental inability to write but from failure to realize the importance of form in communicating the substance of research, in making available for other workers each advance in the discovery of historical processes. Perhaps an application of even a few of these suggestions will make of writing the next paper half the chore and twice the satisfaction.

Division of Historical Research
Carnegie Institution of Washington
Cambridge, Massachusetts
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A CRITIQUE OF DATES AT PALENQUE

HEINRICH BERLIN

DURING THREE field seasons spent at Palenque under the auspices of the Instituto Nacional de Antropología e Historia of Mexico in 1940, 1941, and 1942, I had an opportunity to compare some inscriptions at that great Maya city with interpretations of them published by various students. This convinced me that these interpretations are not always in harmony with the real and actual reading of the inscriptions.

In the following lines I shall demonstrate the result of my analysis, relying on a familiarity with the Maya calendrical system on the part of the reader.

The late Dr. Hermann Beyer[1] claimed that the correct reading of the inscription on the southernmost pilaster of house A of the Palace is:

9.8.15. 0. 0	10 Ahau 8 Tzec
3. 9	deduct
9.8.14.14.11	6 Chuen 19 Pop

For his study, Beyer used the drawing of the inscription published by Maudslay in his *Biologia Centrali-Americana*. We too must use this drawing as well as Maudslay's photograph, since these are actually more complete than the original inscription, which has suffered the loss of whole blocks of glyphs since the days of Maudslay.

I agree with Beyer's reading of glyphs A2, B4, B8, and D3, and I also accept his opinion that the numerical coefficient of the Baktun was probably 9. Thus I agree with the initial tentative reading presented at the beginning of his paper:

9.8.?.?.?	? 8 Tzec
3.9	deduct
9.?.?.?.?	? 19 Pop

But I disagree with his interpretation of the still-existing fragment of the numerical coefficient of the Kin in the Initial Series. Beyer interprets Maudslay's drawing of this particular item as a fleshless jawbone. Now, as the position of the month has already been established (8 Tzec), the Kin of the Initial Series could have been accompanied only by a coefficient of the group 0, 5, 10, or 15. Beyer chooses the value of 0, since he thus arrives at the "round date" 9.8.15.0.0 10 Ahau 8 Tzec, as quoted above. For the "round" reading which he proposes, Beyer himself relies on the frequency of "round dates" or period endings in

[1] Beyer, 1936.

Initial Series. Nevertheless, I want to make clear that it is precisely at Palenque that this common frequency of round dates does not exist, since only one of seven clearly identified Initial Series at Palenque starts with a round date—9.4.0.0.0 on the tablet of the Temple of the Inscriptions. Therefore, it seems that at Palenque people did not grant to round dates the importance given them by people of other Maya cities.

Now, even without considering the foregoing observation, there exists a fact of vital importance: Maudslay's drawing (Fig. 27, *1*) of this particular item is wrong, as can be clearly seen by comparing the drawing and the photograph, both of which are published by Maudslay. Figure 27, *2*, my drawing made from the original inscription, shows

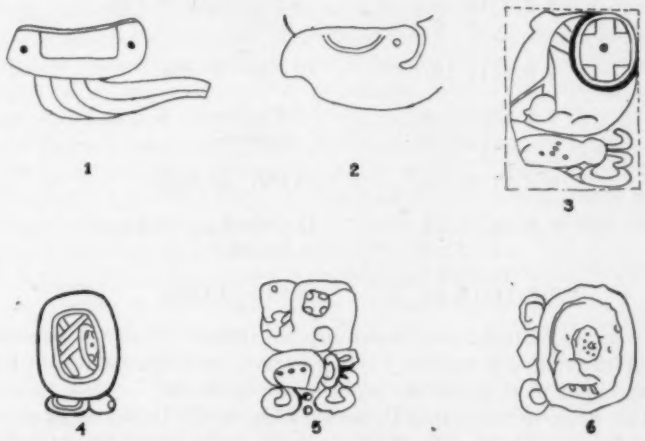


FIG. 27.—*1*, Maudslay's drawing of the remaining fragment of the numerical coefficient of the kin in an inscription on the southernmost pilaster of house A of the Palace at Palenque; *2*, the author's drawing from the original fragment; *3*, drawing by the author of a glyph at Palenque interpreted by Blom as that for Pop; *4*, the glyph for Pop; *5*, the glyph for Kayab; *6*, Maudslay's drawing of a glyph in B13 of the first Secondary Series in the Temple of the Foliated Cross, read by Spinden and Ludendorff as the glyph for Tun.

clearly that this is not a fleshless jawbone and that, as it can correspond only to 0, 5, 10, or 15, as I have shown above, it must be the lower part of the head variant for number 5. Among the faint remains of the obliterated glyph which still exist, one can make out the curved nose of the Old God and the outline of the Tun sign which he wore over his head. Accepting the numerical coefficient of the Kin in the Initial

Series as 5, the corresponding day of the Tzolkin date must be Chicchan —in the Chol language, which is spoken at Palenque, this is the word for rattlesnake. The day of the other calendar round date must, accordingly, be Cib. Thus, the Initial Series can be read as follows:

9.8?.?.5 ? Chicchan 8 Tzec

Unfortunately this is not yet sufficiently definite to furnish us with an exact reading. Four possibilities comply with the above conditions:

9.8.3.15. 5	12 Chicchan 8 Tzec
3. 9	deduct
9.8.3.11.16	8 Cib 19 Pop
9.8.7.16. 5	3 Chicchan 8 Tzec
3. 9	deduct
9.8.7.12.16	12 Cib 19 Pop
9.8.11.17 .5	7 Chicchan 8 Tzec
3. 9	deduct
9.8.11.13.16	3 Cib 19 Pop
9.8.16. 0. 5	11 Chicchan 8 Tzec
3. 9	deduct
9.8.15.14.16	7 Cib 19 Pop

From the material now at our disposal there is no way of ascertaining which reading is correct. Further excavation must uncover at least one specimen more before the problem can be solved.

This demonstration that Beyer's reading of the Initial series can no longer be maintained makes his hypothetical reconstruction of the Supplementary Series of no further value, especially since he uses, for that purpose, series of 6 moons in his computation for glyph C. In another report, in which I published certain new data contained in a paper delivered before the Sociedad Mexicana de Antropología, I have shown that this procedure must be revised.[2]

Another case of erroneous interpretation of a Palenque date is to be found in Blom's *Tribes and Temples*, Volume 1, page 176, where the author refers to an inscription in stucco found on the back of a ruined building. Blom removed these glyphs from their original place and put them in a small museum which he had created for this purpose. In his book, Blom reproduces his drawing of the inscription and writes concerning it: "In glyphs B-5–B-6 we have a secondary series of 13 Tun, 3

[2] Berlin, 1943.

Uinal, and 10 Kin followed by glyph B-7, being End Katun 2. . . . Only in Group G do we find a date which either reads 5 Ahau 18 Pop, or 5 Ahau 18 Kayab. On the strength of what may be 'End Katun 2' given in glyph B-7, Dr. Morley tentatively reads this date as 9-2-3-0-0 5 Ahau 18 Pop."

Of the glyphs found by Blom, some are no longer in the museum: the number of the month is missing, but fortunately the month glyph still exists. As shown in the foregoing paragraph, Blom was undecided as to whether to interpret this glyph as Pop or as Kayab. Finally he accepted the first reading because thus he could establish a relationship between this date and a round one. My drawing of this hieroglyph is reproduced in Figure 27, *3*. Although this drawing, made from the original glyph, is not very good, it is more exact than Blom's. The hieroglyphs for Pop and Kayab are shown in Figure 27, *4* and *5*. Comparison of the glyph under discussion with the glyphs for these two months immediately shows that our hieroglyph is that for Kayab, since it does not exhibit any of the characteristics of the Pop signs—with the exception of the Kin-like element which occurs in both months—but does exhibit many characteristics of the signs for Kayab. There can be no doubt as to the identification of the glyph as Kayab, and consequently Blom's reading of 9.2.3.0.0 5 Ahau 18 Pop must be abandoned. There is not sufficient material here, however, to place the calendar round date 5 Ahau 18 Kayab in its proper position in the long count.

Finally, I wish to examine the reading of the first Secondary Series in the Temple of the Foliated Cross proposed by Spinden and Ludendorff. Both read the Initial Series correctly as 1.18.5.4.0 1 Ahau 13 Mac. With respect to the Secondary Series immediately following, Spinden states: "The situation as regards the first distance number on the Temple of the Foliated Cross (1-14-19) is somewhat better but not entirely satisfactory. After this number we find the declaration of the month position 7 Yax, but the addition leads to 2 Yax. Here it is clear that the coefficient in the day position cannot be changed since it is already nineteen and therefore we must assume that a bar was added to the month-sign which does not properly belong with it.

I. Ser.	1.18.5. 4. 0	1 Ahau	13 Mac
B12–B13	1.14.19		
A14	(1.18.7. 0.19	10 Cauac)	2 Yax."[3]

On the other hand, Ludendorff expresses himself as follows: "If we add to the date of the Initial Series the Secondary Series registered in B12–B13, we reach a day 10 Cauac 2 Yax, although A14 reads 7 Yax. Therefore until today people thought this to be an error. I will . . .

[3] Spinden, 1924, p. 185.

suppose that the indication 7 Yax is meant to indicate the day that follows 10 Cauac 2 Yax by five days.

A3–A9	1.18.5. 4. 0	1 Ahau 13 Mac
B12–B13	1.14.19	
	1.18.7. 0.19	10 Cauac 2 Yax
	5	
	1.18.7. 1. 4	2 Kan 7 Yax."[4]

In other words, both authors interpret the hieroglyph in B13 as 1 Tun. I reproduce this glyph in Figure 27, *6*, taking it from Maudslay's figure. There is no doubt this is, not the Tun sign, but the sign of the day Cauac. Our interpretation 1 Cauac is fully backed by the mathematic relationship existing between the other elements. Thus we read:

1.18.5. 4. 0	1 Ahau 13 Mac
14.19	add
1.18.6. 0.19	1 Cauac 7 Yax

In view of the context of the original inscription, it is clear that the readings proposed by these two authors are without any value and that the reading proposed in the foregoing lines is the correct one.

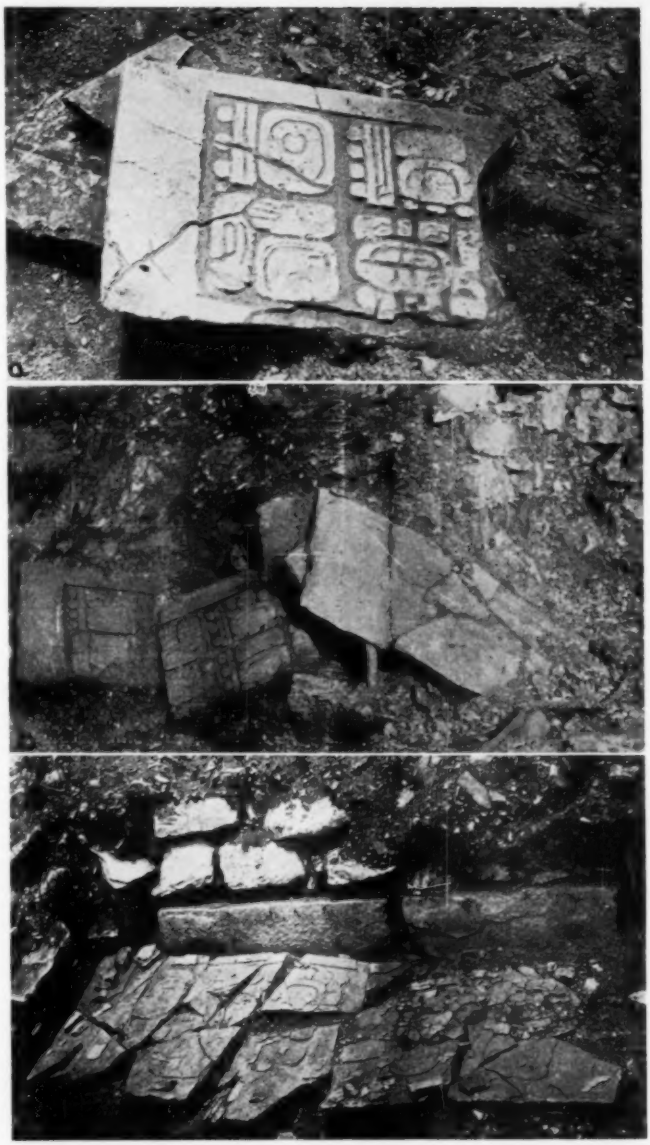
In *Tribes and Temples*, Blom publishes a fragment of an inscription found at the base of the Temple of the Foliated Cross.[5] It is the final portion of an inscription reading 11.12 5 Eb 5 Kayab. Frans Blom, and I think no archaeologist in the world would have done otherwise, considered this as part of an Initial Series, 9.5.1.11.12 5 Eb 5 Kayab.

So far, so good; but new inscriptions found in the Temple of the Sun in 1941 by Miguel Angel Fernández and in the Temple of the Cross by Fernández and the writer make Blom's proposed reading questionable.

When Fernández cleaned the base of the Temple of the Sun, he found on the southern part of the platform dividing the stairway of the pyramid a starting fragment of an inscription (Pl. XXVI, *a*). In A1 and B1 one can clearly read the Calendar Round 13 Cimi 19 Ceh. The glyphs A2 and B2 are not numerical ones. The fact that the Initial Series of the tablet from the Temple of the Sun starts precisely with 1.18.5.3.6 13 Cimi 19 Ceh suggests that the present Calendar Round refers to the same Long Count, although it is of course possible that it refers to another one earlier or later by *n* times 18,980 days. In considering a like inscription in the Temple of the Cross in the following paragraphs, we shall see that a similar inscription undoubtedly refers to such remote times. Therefore, I am inclined to associate also the present Calendar Round with the old 1.18.5.3.6.

Peculiarities in the cutting of the lower part of this slab indicate

[4] Ludendorff, 1938, p. 23. [5] Vol. 1, pp. 191–192.



Fragments of inscriptions from Palenque, Chiapas. *a*, from southern part of platform dividing stairway of pyramid, Temple of the Sun; *b*, from north side of platform near pedestal of balustrade, Temple of the Sun; *c*, from western side of platform dividing staircase of pyramid, Temple of the Cross.

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that originally it leaned against the balustrade of the staircase just over its pedestal. So far, the rest of this inscription has not been found. During the course of the work, various fragments of another inscription with a total of 12 glyphs were found on the north side of the platform, near the pedestal of the balustrade. Unfortunately, only 6 glyphs were legible; the remaining 6 are completely erased (Pl. XXVI, *b*). That the slab formed the decoration of the balustrade on this side, too, is proven by its cutting, which is similar to that of the slab just mentioned.

Omitting glyphs A1, A2, and B1, glyph B2 clearly denotes "end"; C1, 13 Katuns; D1, 8 (Ahau); and C2, 8 Uo. This means that we are here concerned with the date 9.13.0.0.0 8 Ahau 8 Uo, a date which is extremely important at Palenque.

In view of these discoveries, Fernández instructed the writer to look for corresponding inscriptions on the Temple of the Cross. These actually were found in the spot where they were expected to be, on the platform dividing the staircase of the pyramid.

The inscription on the western side (Pl. XXVI, *c*) was assembled from a large quantity of fragments of all sizes, as the big roof stones of the temple, in falling, literally smashed the inscription. To make matters worse, penetrating roots of plants had lifted the upper layers of the limestone slab carrying the relief of the glyphs.

My interpretation of the inscription, based on inspection of the original and on careful measurements of the numerical elements written with bars and dots—in great part destroyed by root growth—follows:

9 Ik	15 Ceh	?	7 Baktuns	11 Uinals	10 Kins
?	?	14 Katuns	14 Tuns	5 Eb	5 Kayab

Readings established beyond question are underlined. As the date 9 Ik 15 Ceh which occurs also on the tablet of the Temple of the Cross is linked there with the Long Count 1.18.5.3.2, I think it should be linked in a similar manner in the present case. The date 5 Eb 5 Kayab occurs in the Death's head monument near the Temple of the Foliated Cross. From the relationship which it has there to the date 8 Ahau 8 Uo —the end of Katun 13—it is clear that it belongs to the Long Count 9.12.19.14.12. Therefore, we can ascribe the date 5 Eb 5 Kayab, in the present case, to 9.12.19.14.12. Ascription of the two Calendar Rounds to these Long Counts is confirmed by the interval denoted by the remaining decipherable glyphs which connect the two dates:

1.18. 5. 3. 2	9 Ik 15 Ceh
7.14.14.11.10	add
9.12.19.14.12	5 Eb 6 Kayab.

It is true that the numbers of the Baktun, Katun, and Tun can no

longer be read with exactness, but the remaining traces of the numbers are sufficient to prove that the numerical coefficient of the Baktun was either 7, 8, or 9; that of the Katun, between 11 and 14; and that of the Tun, either 12 or 14. Our suggested reading is a possible one. On the other hand, no other possibility can link the two Calendar Rounds, although they may be connected with some other Long Count. Therefore, I believe that my interpretation is well founded. We need not wonder at the fact that the Secondary Series in this case is written in the style of an Initial Series starting with Baktuns and ending with Kins, as a similar case occurs also on the tablet of the Temple of the Foliated Cross. If it were an ordinary Initial Series with a numerical coefficient of the Kin as 10, the day of the Calendar Round could never be Eb—it would be Oc. But the numeral of the Kin as well as the face of the Eb is beyond doubt, making it impossible for the Secondary Series to be an Initial one.

Concerning glyphs C1, A2, and B2 nothing can be said, but it should be noted that glyphs B2 and C1 precede the date 9 Ik 15 Ceh on the inscription of the tablet, indicating that in the tablet too there exists a close relationship between this Calendar Round and the said two hieroglyphs. This can be considered further proof of the correctness of our identification of the date of the new slab with the known one on the tablet.

The hieroglyphic slab corresponding to the other balustrade was found under the eastern pilaster of the main doorway of the temple, which had fallen exactly on top of the stone, and covered it. It was extracted from this position personally by Fernández, with positive risk to his life, as there was danger that the pilaster would break after the removal of the slab which had partially supported it.

Although it is apparently in a better state of preservation, it is badly defaced. This makes the reading difficult, particularly as none of its 12 glyphs is a numerical one, but all belong to the group which is as yet poorly defined. One can only stress the fact that glyphs E1 and F1 are repeated in identical manner in the tablet of the Cross in K5 and K6. So it would seem that this slab was intended to impart information which appeared on the tablet, as was true in the case of the previously mentioned slabs. These slabs, owing to their position, must have been seen by all the common people, while the data on the tablets certainly were considered as esoteric.

Now the inscription which Blom found on the base of the pyramid of the Temple of the Foliated Cross has the typical cutting which we have seen in the four slabs just discovered, showing that it too originally leaned against the balustrade of the stairway. The style and the simple frame are of the same workmanship as appear in these slabs. We have seen that these were composed of twelve glyphs, and hence it is logical

to suppose that the corresponding ones of the Temple of the Foliated Cross also had twelve glyphs, particularly if the close relationship which exists between the three temples be considered. Blom's reading would give room for only eight glyphs. As long as a portion of the slab found by Blom is missing, one cannot prove beyond question that Blom's proposed reading is wrong, but I think that the new discoveries render his reading open to doubt and make it much more probable that the Calendar Round 5 Eb 5 Kayab refers to our 9.12.19.14.12 and that the 11 Uinals 12 Kins are the last hieroglyphs of another Secondary Series, with another date still unknown. There is good reason to hope that during a new field season at Palenque the missing part of the Blom fragment may be found and also that the corresponding slab of the other balustrade may be discovered.

Since this was written Mr. J. Eric Thompson has informed the writer that he has already corrected the reading of the first Secondary Series in the tablet of the Temple of the Foliated Cross in *The Solar Year of the Mayas at Quirigua* and that Beyer had corrected it still earlier. In 1938 Ludendorff was still using incorrect data; it seems advisable to publish the correction once more so that it may be circulated.

Thompson also pointed out that the Kayab of Figure 27,*3* has the typical accompanying flame element which Kayab normally—but Pop never—has, thus strengthening our choice of Kayab.

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Mexico, D. F.
December, 1943

TREPANATION AND THE "CATLIN MARK"

WILLIAM M. GOLDSMITH

THIS PAPER deals with the confusion of a certain anomalous condition of the human skull with one type of trepanation. Evidence is presented to show that many specimens exhibited as examples of a certain type of trepanning are clear-cut cases of this congenital bilateral fenestra in the parietal bones. As is generally known, trephining was performed by ancient man[1] to relieve certain maladies such as headaches caused by "demons kicking up a fuss" inside the skull. The excruciatingly painful operation was performed with a crude knife, piece of glass, or sharp-edged stone. Trephining probably flourished in Neolithic times, especially in Bohemia and western Europe. Trephined skulls have been reported from Bolivia, Central America, Mexico, Peru, and various other localities.

Since the knowledge of trephining has become common and the finding and reporting of trephined skulls is very popular, the tendency is to label nearly all skulls that appear old and which have holes of almost any sort as cases of trephining. Anomalous skulls may be seen in some of our greatest museums (Pl. XXVII, *a*), as well as in private and small college exhibits.

In the legend accompanying the "trephined" skulls in one museum, we read, "The trephine hole is usually located on the upper and posterior part of the parietal bone." Of the fourteen so-called "trephined" skulls in another museum, only four showed the holes in the parietals, while eight were clearly "trephined" in the frontals. The other two skulls were so irregularly marked that they could not be classified on this basis. Though the number is too small to warrant broad generalization, it is obvious that the trephine hole is not "usually" in the parietals.

While reviewing many so-called "trephined" skulls, as well as records in the literature, I became aware that in a considerable number of those having bilaterally "trephined" parietals the fenestrae were round to oval, *in about the same position*, and bore a remarkable similarity in different skulls, *though their proveniences might be widely separated parts of the globe*. Since these aborigines of thousands of years ago certainly were not in direct communication with each other, it seems improbable that their primitive surgery would follow so constant a plan. On the other hand, if these quite uniformly paired parietal fenestrae were produced by nature, we would be no more surprised at their similarity than we are at the similarity in cases of syndactylism, brachy-

[1] "Trepanation," *Ciba Symposia*, Vol. I., No. 6, Sept., 1939.

dactylism, or any other human mutation or sport. We therefore propose these queries: Are these so-called "trepanations" not individual or familial anomalies comparable to other variations which characterize *Homo sapiens?* Were these two holes in most skulls not produced by nature rather than by some cruel, but well-meaning, medicine man or witch doctor? Was not the trephiner Mother Nature herself? What evidence have we for proposing such radical questions, an affirmative answer to which would cause many skulls to be removed from trephine cases?

The writer and many other investigators have published papers, bearing such titles as "Catlin Mark," "Fenestrae Parietales Symmetricae,"[2] etc. These papers show scores of cases where individuals were born with paired holes of varying sizes in the parietal bones (Pl. XXVII, *e*). As another instance of a coincidence in scientific literature, the writer, in America, and Neurath,[3] in Europe, reported similar cases in 1922. Pepper and Pendergrass, in a paper in 1936 dealing with the inheritance of unusual openings in the parietal bones, say, "Goldsmith had an opportunity to examine fifty-six members of the Catlin family (Pl. XXVII, *e*) distributed in five generations; sixteen exhibited the anomaly."[4] In the same year, Neurath described two cases of hereditary parietal fenestrae in two European families—one, a mother and her only child, and the other, a mother and two of her three children. There is little doubt that other members of these families possessed "eyes" in the back of the head, but the European worker did not have an opportunity to trace the family histories as I did with the Catlins.

In a recent publication, entitled "Bilateral Fenestrae in the Parietal Bones,"[5] I have described or reviewed other specific cases. It will suffice to review and illustrate two or three cases to show that numerous skulls in various exhibits of trephination are in reality examples of this strange human anomaly and should no longer retain their erroneous labeling.

Plate XXVII, *e* is a photograph and X-ray of the back of the head of a typical subject possessing the characteristic "Catlin Mark," or parietal fenestrae. The depression over each hole may be seen in this subject from many feet away. As this and other figures show, the openings are on either side of the sagittal suture. The location within the parietals varies somewhat with the individual.

[2] W. M. Goldsmith, "The Catlin Mark," *Journal of Heredity*, Vol. 13, (1922), pp. 69–71. [3] R. Z. Neurath, *Kinderheilk*, Vol. 32 (1922), p. 121.

[4] Pepper and Pendergrass, "Hereditary Occurrence of Enlarged Parietal Foramina," *American Journal of Roentgenology*, Vol. 35, No. 1, 1936.

[5] *Journal of Heredity*, September, 1941.

Plate XXVII, *f* is an X-ray of a patient in Iowa University Hospital. This was kindly submitted by Dr. Carl L. Gillies, M.D., of the Department of Radiology. Plate XXVII, *b*, *c* are roentgenograms of the "Catlin-Marked" skull of an eleven-year-old Negro boy reported in November, 1940,[6] by Doctors Warkany and Weaver of Cincinnati. The increasing number of cases reported in recent years shows that this strange human anomaly is not uncommon.

The writer concluded in an earlier paper, as have other investigators, that these bilateral openings in the parietal bones were merely enlarged parietal foramina through which the emissary veins passed. Certain specimens, however, tend to show that two separate factors determine the presence of and variation in the normal foramina and the "Catlin Marks." They have different origins, though anatomically they oftimes, if not usually, fuse or, rather, are superimposed.

In another specimen, shown in Plate XXVII, *d* the right fenestra is in contact with the sagittal suture, a condition which is somewhat unusual. This calvarium is exhibited in the halls of Tulane Medical School. According to Professor Harold Cummins of the Anatomy Department of Tulane University, "the calvarium is from a ***dissecting-room cadaver***, and it has no recorded history because its interest was discovered only after all the skull caps and bodies were dissociated." This otherwise well-shaped skull encased the brain of an individual who evidently walked the streets of New Orleans only a few years ago. Since this and the X-rayed skull of the living man shown in Plate XXVII, *e* as well as numerous other reported cases (Pl. XXVII, *b*, *c*, *f*), are almost identical with the exhibited ancient so-called "trephined" skulls, one type of which is shown in Plate XXVII, *a*, it seems likely that the numerous ancient skulls of the "Catlin Mark" type (Pl. XXVII, *b*, *c*, *d*, *e*, *f*) are cases of human skull anomalies and ***not*** examples of trepanation.

The skull illustrated in Plate XXVII, *a*, labeled "trephined" in one of our museums, best illustrates the point in hand. The position, size, and shape of the two holes are quite comparable to those of holes in the skulls of the living (or recently deceased) individuals referred to above (Pl. XXVII, *b*, *c*, *d*, *e*, *f*). The mere fact that the skull was found, perhaps with other, normal, skulls, in some remote Indian burial mound would be no indication that it was "trephined." We might conclude, as logically or, on the basis of our present knowledge, even more justifiably, that the individual was born with holes in his skull.

If some prankster had dried the green Tulane skull (Pl. XXVII, *d*), treated it with some strong chemical, rubbed it with ashes and clay, and

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[6] *American Journal of Diseases of Children*, Vol. 60.

otherwise given it an ancient look, and had then offered it to some one of our colleges with the claim that it was found in an Indian mound of Central America, it would no doubt have found a place with many other mis-labelled specimens in a "trephine" cabinet. The writer has heard many commercial guides glibly recite to thousands of sightseers how these so-called trephined skulls of "Catlin Mark" type, thousands of years old, were drilled or chiseled with crude instruments by an ancient medicine man to release the spirits that were "kicking up the devil" inside the head of the suffering patient.

Dr. Gillies of the Department of Radiology, Iowa University Hospital, referring to the differential diagnosis between healed recently trephined wounds and congenital openings says, "There is no difficulty in making the distinction between radiographs of trephined skulls of today and those showing the 'Catlin Mark.' Trephine openings now are made with a burr and the openings are true circles with straight walls (Pl. XXVII, *h*). The 'Catlin Mark' is not a true circle and the margins are thin. It is conceivable that trephine openings such as would be made by ancient Indians could be more easily confused with the 'Catlin Mark' since they would not be likely to be circles and the margins might be thin."

The skull illustrated in Plate XXVII, *g*, without information as to origin, age, or race, is classed as a "trephined skull." We have little evidence, if the frontal fenestra be not considered, that the skull is of ancient origin. There seems to be little serious objection to assuming that the individual died only a few years ago.

One museum has no less than a half-dozen skulls labeled "trephined" all of which are quite similar to the one at Dubuque University (Pl. XXVII, *g*). One cannot refrain from asking how it would be possible for so many skulls from such diverse localities to bear such a striking resemblance. The opening is almost universally single and slightly to the right or left of the midline. The variation in size and position of the frontal fenestra seems about comparable to that of the "Catlin Mark." Is it not possible that future research will demonstrate that many such frontal apertures were caused by incomplete ossification, as is clearly shown to be the case in many cases of so-called trephining of "Catlin Mark" type?

Also, we must not overlook the possibility that some skull openings have been caused by disease. In fact, one specialist who examined the skull shown in Plate XXVII, *g* says, "There are so many diseases that produce defects in the skull that, to me, disease seems a more logical cause."

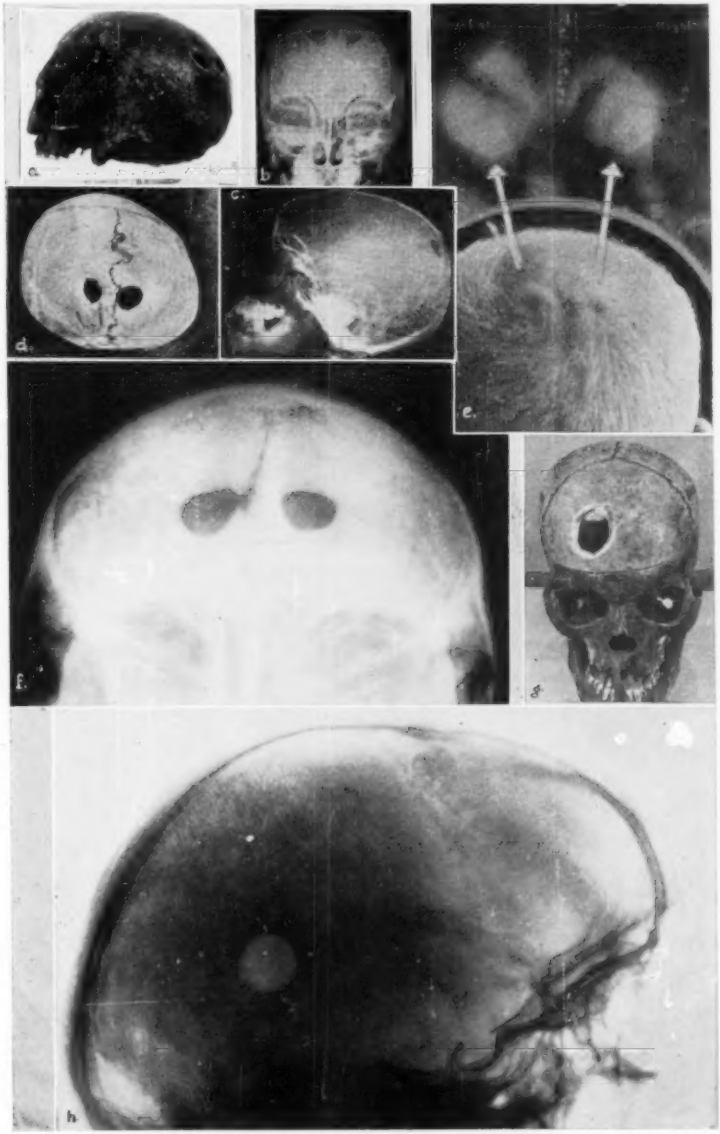
My thesis as to the origin of such perforations in skulls can be

checked by a tabulation of all possible living individuals bearing "soft spots" on their heads. Already, sufficient living individuals bearing the typical bivalent holes in the parietals have been studied to justify relabeling or removing from exhibits almost all so-called trephined skulls of "Catlin Mark" type. In like manner, if numerous living cases can now be found having frontal fenestra from birth, the exhibit cases will again shrink. Even if it should be established that many such skull anomalies are mistaken for ancient trephining, such cases would not militate against the fact that trephining was practiced by many primitive peoples.

Central College
Pella, Iowa
December, 1943

LEGEND, PLATE XXVII

Photographs and roentgenograms of skulls showing trephining and anomalies. *a*, an ancient skull of the "Catlin Mark" type. There is little doubt that the bilateral holes were in the parietal bones of this skull at birth and were not drilled or chiseled; *b*, *c*, roentgenograms of the skull of an eleven-year-old Negro boy showing Fenestral Parietals Symmetricae (Halbertsma's terminology, "Catlin Mark" of Goldsmith); *d*, a calvarium illustrating the "Catlin Mark" (Fenestral Parietals Symmetricae) exhibited in the halls of Tulane Medical School; *e*, photograph and X-ray of living subject showing a typical "Catlin Mark." The parietal fenestrae of such living individuals are identical with the numerous cases of ancient "trephining" like that illustrated in *a*; *f*, a recently discovered case of "Catlin Mark" or "Congenital Trephining"; *g*, a frontal fenestra of the type usually identified as trephining, an identification which the author questions. The specimen is from the University of Dubuque museum; *h*, X-ray showing case of recent burr trephining after healing had taken place.



Photographs and roentgenograms of skulls showing trephining and anomalies.
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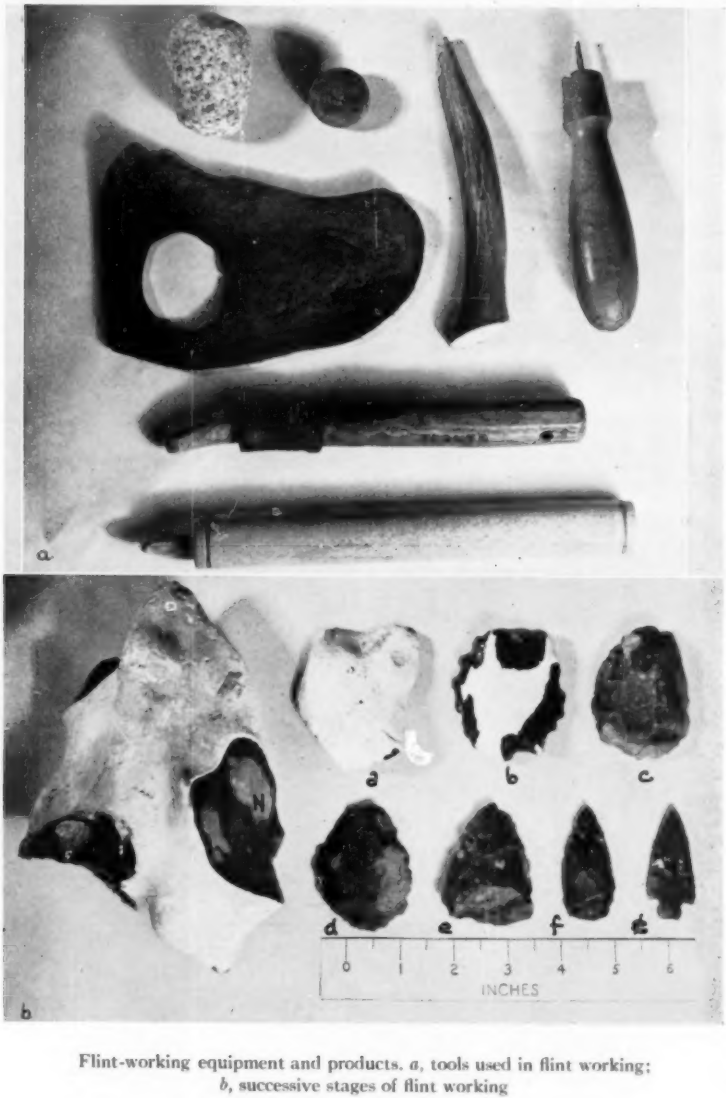


Flint-working equipment and products. *a*, tools used in flint working; *b*, successive stages of flint working

FLINT CHIPPING[1]

LAWRENCE B. BIXBY, COL., U.S.A.

ONE DAY, in the spring of 1937, while looking at some Indian artifacts in Peabody Museum at Harvard University, I saw a card of flaked stone objects that had been made by Mr. F. R. Whitney of Tacoma, Washington. The card stated that more information concerning these pieces might be obtained in the Museum Library. I promptly went to the Librarian and asked about them. She told me that if I were interested in the manufacture of stone arrow points I should get in touch with Mr. Charles E. Snow, who was then a student of anthropology at the University.

When I found Mr. Snow, he told me that he had worked at flint chipping as a hobby to some extent. Dr. Earnest A. Hooton encouraged his students of anthropology to work with flint in the laboratory, and gave a prize each year to the student who did the best work. Mr. Snow had won the prize the year before.

Later I talked with Dr. Hooton and learned that most of his students made little progress in this type of work. He considered it worthwhile, however, as it developed an appreciation for the lengths to which the art had been carried by primitive peoples. He was very encouraging and gave me sufficient Brandon flint to start some work on my own. During an evening or two with Mr. Snow I learned a little of the methods of approach, then set to work.

Materials were hard to get in Boston, but by exploiting all sources I was able to keep my supply of raw material replenished. In the ground around old Indian sites I was able to find pieces of stone suitable for the manufacture of small points. In a pinch, it was always possible to find thick glass in the form of insulators, bottles, and pieces of broken automobile windshields.

Perhaps the most remarkable find of material occurred while Mr. Snow was on a noon-time jaunt in Chelsea, Massachusetts. In a vacant lot which was being rapidly filled as a dump he found many large nodules of Brandon flint. The identity was unmistakable, as it was the same stone as that purchased each year from Brandon, England, for use in the laboratory at Harvard. How the flint got into a back lot in Chelsea is something of a mystery. The only explanation that has been offered is that it was brought over in old sailing vessels as ballast and dumped along the former waterfront. At any rate, we secured enough flint to keep Mr. Snow and me, as well as the laboratory at Harvard, supplied

[1] Shortly before this issue of AMERICAN ANTIQUITY went to press Colonel Bixby went overseas. He has not, therefore, had an opportunity to check proof of his article. Ed.

for the next year. During a trip to Missouri, Mr. Snow shipped a keg of chert to me. It arrived labeled "one keg of common rock," to the disgust of the freight handlers.

I found flint chipping a fascinating hobby, good exercise for the hands and arms, and habit-forming, like whittling. It was hard to stop. My work extended intermittently over a period of about three years and ended only because of the press of my military duties.

On only one occasion have I had an opportunity to make a practical application of flint chipping. During a visit to the Wayside Inn, famous as the home of Longfellow's "Tales of a Wayside Inn," I was shown many interesting antiques. Among them was a device for lighting a fire. It consisted of a pistol grip, with a flint and steel mechanism similar to that employed in old flintlock muskets. In place of the gun barrel was a holder for the tinder. The tinder was ignited by sparks when the trigger of the flintlock was pulled. The hostess who described the fire lighter deplored the fact that she had no flints and was therefore unable to demonstrate how it worked. When I went home I made a few gun flints of various sizes and mailed them to the Wayside Inn. A letter of thanks assured me that the fire lighter was then in full working order.

My last arrow point was made in the command post of the 1st Field Artillery Regiment, while I was on maneuvers in Georgia in the spring of 1940, to win a small bet. It so happened that the command post was located on the site of an old Indian village or camping ground. I could tell this by the numerous flakes of chert and the fire stones that were scattered about. During a rather dull Sunday afternoon the officers were talking about the former Indian inhabitants of our camp, and I mentioned that I had made arrow points. A bet was placed to substantiate the general feeling that I was not telling the truth. With a fair-sized piece of flint from the Indian camp, and with an old rusty ice pick, from which I removed the temper by heating in the kitchen fire, I managed to chip a satisfactory arrow point in fifteen or twenty minutes.

The object of my study was to determine how chipped artifacts might have been made by primitive men, the time element involved, the mistakes and errors that led to broken and unfinished products, and, in general, to gain an impression of the art of the arrow maker. In the following pages I have set forth what I have learned by experiment and practice. The methods explained should enable anyone with sufficient strength and enthusiasm for the work to reach the point where he can successfully make arrow points and similar artifacts. Many failures must be expected at first, as well as sore muscles, blisters, and cuts, but, after a few weeks of daily practice, progress is rapid and twenty minutes of flaking should produce a fairly good finished piece.

It must be understood that my purpose in preparing this monograph is not to point the way to the manufacture of spurious artifacts. It is, on the contrary, a sincere effort to record what I have learned in seeking to duplicate the beautiful workmanship of primitive peoples in what is practically a lost art. It is hoped that my notes may be of some help to others who wish to experiment in this field. If they serve no other purpose, they may increase the appreciation of the difficulties that were overcome by the primitive craftsman in producing the tools of his daily life and his works of art.

I wish to thank Dr. Hooton and Mr. Snow for their early encouragement and Mrs. Ruby L. Budden for her help in preparing the manuscript.

MATERIALS

The materials from which chipped implements may be made must have one essential quality, namely, a conchoidal fracture. Minerals that are fine-grained and homogeneous break, when struck or chipped, in such a way that the place where the break occurred has a shell-shaped appearance, like the inside of a clam shell. Correspondingly, the piece that is thrown off has the appearance, though sometimes irregular, of a small sea-shell.

The conchoidal fracture is a characteristic of the glassy minerals. We find it in obsidian, flint, chert, jasper, chalcedony, quartz, quartzite, and in artificial glass. As the characteristics of these minerals are important to the flint worker, they are described briefly. They are listed in the order of their relative merit as materials for chipping.

Obsidian. This is a black natural glass formed from the rapid cooling of lava. It is composed largely of feldspar and quartz and has, therefore, a very high percentage of silica. The minerals are the same as those which form granite, but the cooling process has been too fast to permit crystallization. This makes the obsidian remarkably homogeneous and gives it a beautiful conchoidal fracture that enables the flint chipper to produce very long thin flakes, with a resulting fineness and finish to the artifacts.

Obsidian was highly valued by primitive people. The flakes were razor keen and could be used as cutting tools without further preparation. It was widely used by the Indians of Mexico, as the mineral is plentiful in southwestern United States and Mexico.

Obsidian is harder than window glass, yet it seems less brittle. Fewer accidental breakages occur when working with obsidian. The flakes are more easily controlled, and in every way it is a superior material for the beginner or the expert.

Artificial glass. Although this is not a natural mineral, it is included because it is the most easily available material for anyone who wishes to experiment with flint chipping. As a matter of fact, it has been said that certain primitive people in South America have stripped the glass insulators from electric transmission lines for the purpose of making chipped implements.

Glass is relatively fragile, and the principal difficulty for the beginner will be frequent accidental breakages. Pressure with the flaker, improperly applied, will crack the arrow point squarely across, perpendicular to its axis.

Window glass is of no use, as it is too thin. Even the heaviest plate glass from automobile windshields is too thin for satisfactory work, except to make very small artifacts such as the so-called bird points.

Glass of suitable thickness may be obtained from the bottom of bottles, from glass insulators, or from chunks of melted glass. Bottle glass makes very beautiful implements, as a variety of colors may be obtained in the dumps and refuse piles that abound in and about every city and town.

Flint, chert, and jasper. These minerals are formed by deposition from liquids containing colloidal silica. In some cases, the silica has entered the solution from the bodies of small sea organisms and has then been deposited as flint nodules in chalk. In other cases, large masses of chert have been deposited in thick masses and beds.

While quartz is crystallized silica, the flints and the cherts are not apparently crystallized. They contain more or less combined water and certain impurities like clay, iron oxides, etc., which give them their distinctive color. Flint is a dark gray or black rock, so fine-grained that it appears to be homogeneous. Chert is lighter colored, commonly white, gray, or blue-gray. Jasper is red or reddish-brown chert.

Flint is an easier material to work than chert, which varies greatly in its qualities of toughness and brittleness. Some types have a good clean fracture and work smoothly, while others are stony and brittle, cracking and breaking irregularly or else defying all attempts to work them.

It is my impression that chert taken freshly from the ground or quarry is easier to work than chert that has been exposed to the air for some time. If it is possible for this stone to "dry out," then I believe that the drying-out process makes the stone harder to work. I have never been able to test this scientifically. I did have one large piece of chert that I obtained from a quarry below water level in Lake Champlain. It worked easily and nicely when fresh, but a year later I could do nothing with it. I have read of many caches of "turtlebacks" (partially worked stone) being found under ground near springs used by the

Indians of New York State. These caches were thought, at one time, to have some sacrificial or religious significance, but I feel that the Indians were attempting to preserve the moisture in the materials that had been laid away for future use.

Quartz. This is a pure silica in crystallized form. It has a conchoidal fracture but is inclined to be uneven and splintery in some varieties. I have seen some beautiful artifacts made of quartz, but I have been unfortunate in having only the splintery or the brittle kinds, such as are found in New England. It is a poor material for the beginner, as it requires considerable strength and skill to chip and shape it.

Quartzite. This is a metamorphosed sandstone. It consists of grains of quartz sand united by a cement of quartz. In the finer varieties, the sand and the cement are of equal strength, so that a true conchoidal fracture is produced. It works well enough, and quite easily, but there is considerable variation in the quartzite from different localities.

Petrified wood. This mineral has an excellent fracture, limited by layers of different texture. I have had a very small amount of it. My experience has been that certain layers give a fracture as good as that of obsidian. If a homogeneous piece can be obtained from a single layer, a very fine implement can be produced.

Old arrow points. For the beginner, I strongly recommend working on old arrow points for practice. Broken points may be reshaped, for example, or large ones made smaller. Finishing processes, such as notching, straightening edges, and shaping, may be practiced in this manner without the tedious approach of working down from the large rough blocks. I have put to use in this manner many unfinished implements and cast-off flakes picked up on old Indian sites.

TOOLS

The only tools needed for the manufacture of arrow points, drills, and scrapers are hammerstones, flakers, and a leather pad to protect the hand (Pl. XXVIII, *a*). It is highly desirable to wear glasses while chipping flint to protect the eyes from occasional fragments that might do serious damage if they struck the eyeball.

Hammerstones. Any suitable field stone may be used as a hammerstone. It should be some compact, hard stone, such as a fine-grained granite or felsite. The shape should be convenient to hold between the thumb and fingers while the stone is in use. Flat circular stones are particularly good. Hammerstones may be picked up in abundance in almost any stream bed or gravel pit and may be tried and discarded until the most convenient sizes are found.

Flaker. The flaker is used for chipping off flakes in the final stages of

manufacture. It must be of some tough material, soft enough for the flint to bite into, but hard enough to withstand the wear and tear of working without being worn away too rapidly. The best material is green antler. When the antler becomes old and dries out it is so brittle that it wears away very rapidly.

A simple flaker may be made by cutting off six to eight inches of the end of a deer antler and sharpening the tip to a chisel point so arranged that when the antler is held in a natural position in the hand the sharp edge will be vertical. A hack-saw will cut the bone very neatly, and a file will sharpen it.

A better grip may be obtained if the end of the antler is cut off to a length of about three inches, trimmed down until it is circular, and set in the end of a wooden handle so that about one inch protrudes. This gives a larger handle and a better grip. If the antler is fitted as closely as possible into a hole in the end of a hardwood handle, it may be set with plastic wood or some heavy glue so that it will hold quite firmly.

For very fine work on small arrow points and for notching, a small flaker is necessary. This may be made of a small piece of antler cut or filed down and set in the end of a tool handle.

For those who are more interested in the production than in the method, a soft iron nail in the end of a wooden handle will make a very good tool. A nail, about three-sixteenths of an inch in diameter, may be driven about two-thirds of its length into the end of a wooden handle. The head may then be cut off and the end sharpened to a chisel point. The nail is relatively soft and will not slip against flint. A steel tool is useless, as it is too hard to be cut by flint and will slip off the stone.

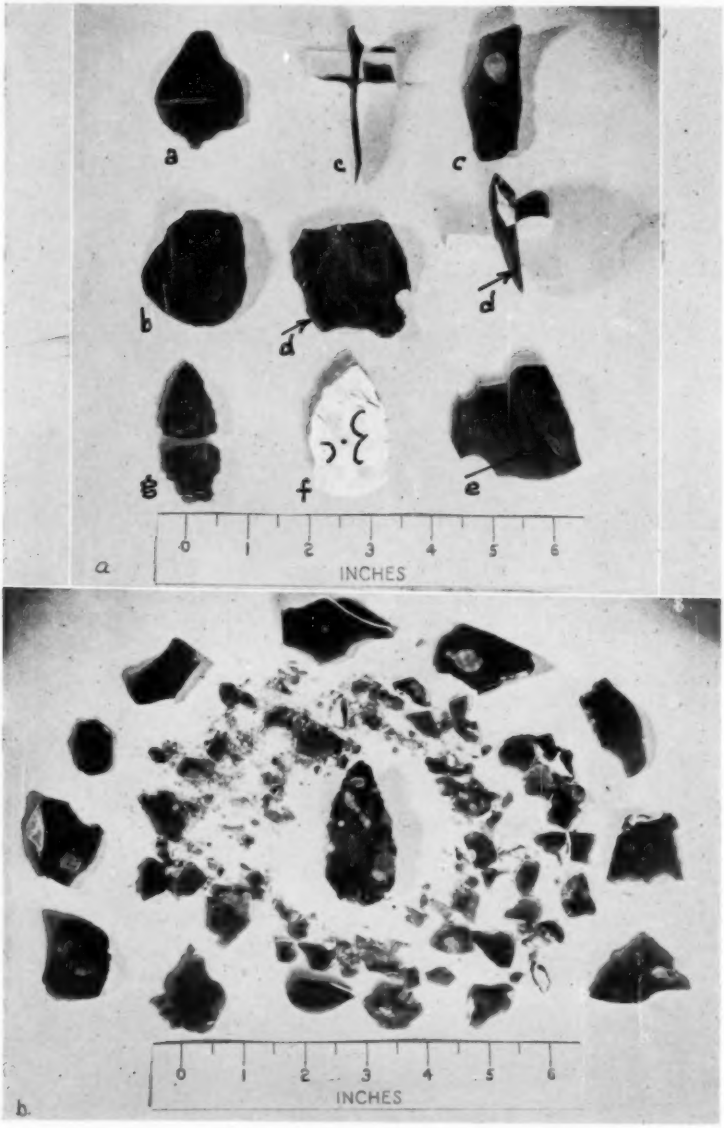
Leather pad. A leather pad is necessary to prevent the flint chips from being driven into the palm of the left hand. It is convenient to cut a hole in the leather so that the pad may be hung over the left thumb and thus be kept in place. Stiff leather such as that in an old boot top makes a good pad.

The work shop. Some thought must be given to the place in which the work is to be done. For working indoors it is desirable to have a large tray to catch the flakes. If the work is done outdoors, it should be in some secluded place where barefooted children will not run through the sharp flakes. The flakes often have razor-like edges.

METHOD OF WORKING

Select a nodule or chunk of stone and prepare a striking platform. A striking platform is simply a plane surface more or less perpendicular to the axis of the stone. The angle between the striking platform and the remainder of the stone should be about 90 degrees (Pl. XXIX, *a*).

Flint working. *a*, use of the hammerstone; *b*, use of the flaker.



Flint-working products. *a*, examples of the difficulties involved; *b*, a piece of worked flint with the flakes thrown off in the process of manufacture.

Select a surface of the nodule from which you intend to strike a flake. Hold the nodule in such a way that the proposed flake is supported by the fingers of the left hand. The more firmly the nodule is supported at the point where the flake is to be thrown off, the longer and thinner will be the flake.

Take a large hammerstone in the right hand and deliver a smart blow on the striking platform near the edge of the side from which the flake is expected to fall (Pl. XXIX, *a*). A sharp snap of the wrist is better than a full-arm swing. Experience will teach you the amount of snap to put in the blow. A conchoidal flake will be thrown off.

This particular step will require considerable practice before flakes which are of suitable size, yet thin enough for future working into arrow points, are produced. Once a suitable flake has been obtained—let us say one that is about three inches long, two or three inches wide, and about one-half inch thick—the second step in the process maybe started.

Select a hammerstone that weighs less than the piece upon which you are working. This principle should be followed throughout. When the hammerstone weighs more than the piece, it has a tendency to crack the piece straight through the middle rather than to throw off conchoidal flakes. The object of the next step is to obtain a sharp edge all the way around the flake, and in a plane perpendicular to the smaller axis, by striking blows with a hammerstone on the appropriate side of the piece, always supporting the proposed flake with the fingers or the palm of the hand. In this way, the piece may be given a proper edge for future working. The flakes thrown off are large, becoming smaller as the process continues through succeeding stages (Pl. XXVIII, *b*, b; Pl. XXX, *b*).

Once the edge is established, proceed around the piece with small taps of the hammerstone, first on one side and then on the other, turning the stone back and forth to keep the edge in a single plane and to throw off flakes on both sides of the stone. The result of this process is a more or less flat, oval piece of stone, chipped all over and with a fairly regular sharp edge all the way around (Pl. XXVIII, *b*, c). This is commonly known as a turtleback. Many native peoples worked the stone to this point at the quarry and then transported the turtlebacks to the village sites, bartered them, or stored them for future use. If the process has proceeded safely this far, the chances are that there are no flaws in the stone, and it is a matter of just a few minutes' work to complete a drill, a scraper, a knife blade, or an arrow point.

If the manufacture of an arrow point is intended, the turtleback is worked to the shape desired; the process is begun with hammerstone as in the last step. Better control of the flakes is obtained by using smaller hammerstones as the work proceeds (Pl. XXVIII, *b*, d and e).

When the desired shape is approached, the hammerstone is abandoned and work is started with a flaker. The work done with the flaker is called secondary chipping. Use the leather pad on the left hand. Place the piece on the heel of the left hand, or in the palm, and clamp it firmly with the fingers of the left hand (Pl. XXIX, *b*). The greater the pressure exerted by the left hand while throwing off the flakes, the longer and thinner will be the flakes. Select a point on the sharp edge of the piece, hold the flaker so that the chisel edge is perpendicular to the edge of the piece, and press the flaker firmly against the flint. Continuing this pressure in the direction of the flaker, sharply bend the wrist to force the point of the flaker down against the hand and towards the center of the palm. A very sharp, decisive wrench must be given. When it is done properly there is a sharp click as the flake is thrown off, and the flakes will be long and thin. If the stone merely crumbles or breaks in a number of very small pieces, it is because the longitudinal pressure on the flaker has been too great and the snap to the wrist has not been great enough.

The directions of pressure are shown in Plate XXIX, *b*. The fingers of the left hand press down firmly, causing the flake that is to be thrown off to be firmly supported by the heel of the hand. The flaker is pressed strongly against the edge of the flint. While these pressures are maintained, the hand is bent sharply downwards at the wrist, snapping the flake from the flint.

Having thrown off the first flake, place the point of the flaker about a quarter of an inch further along the edge of the piece and throw off another flake. Now turn the piece over and throw off a flake in the opposite direction, applying the same pressure to the point immediately between the two previous flakes. By working on alternate sides in this manner, proceed around the edge of the piece, shaping the arrow point as desired.

While working near the nose of the arrow point, it is best to direct the pressure of the flaker diagonally toward the opposite corner of the base. This prevents breaking the nose straight across and spoiling the piece, and generally makes for better flaking.

The final step in the process is notching the arrow point. Hold the point in the left hand as before and start at one corner of the base, using a very fine-pointed flaker. Work in toward the center of the point by throwing off flakes first on one side and then on the other. During this process, accidental breakage is very likely to occur, due principally to the application of too much pressure at a place where the point is not properly supported by the palm of the hand.

This trouble may be prevented by holding the point between the thumb and forefinger and working against the thumb, particularly with very small arrow points. Notching the point will require considerable

practice before it can be done successfully every time. The edges may be left serrated or they may be smoothed out by careful work with the point of the flaker.

DIFFICULTIES ENCOUNTERED

In spite of the best efforts, certain difficulties will be encountered. These are due to faults in the stone and faulty methods of working. The objective, as I have pointed out, is to cause long thin flakes to be thrown from the stone. If the part of the stone that is to be thrown off as a flake is firmly supported when the blow is struck or the pressure is applied with the flaker, the tendency is for the flake to be longer. An example of good flaking is shown in Plate XXX, *a*, c. These flakes are long and thin.

Hinge fracture. When a strong blow is struck with the hammerstone, a wave action is set up in the stone which frequently causes ripples along the break. These are clearly visible in the specimens shown in Plate XXX, *a*, b and c. Ordinarily these ripples cause no trouble, but occasionally they result in the flake being thrown off with a rounded terminal edge (Plate XXX, *a*, d). This is called a hinge fracture. These rounded edges are so smooth and so hard that it is almost impossible to flake them. If continued work on the flake is intended, it is necessary to remove the rounded edge by gradually flaking it away from the ends. On the other hand, if the concave side of the hinge fracture is left in the piece upon which you are working, there is no way to remove it, and it may spoil the finished product. The concave side of a hinge fracture is shown in Plate XXX, *a*, e. Hinge fractures are accidents beyond the control of the worker, as they probably are due to some fault in the stone.

Peak or high point. The beginner will frequently find that his flakes are not long enough to cross half the width of the piece he is working on. Continued working around the edge will result in a peak or high point on one surface of an arrow point. Such a peak, shown in Plate XXX, *a*, f, is represented by a black dot. Flakes terminate as shown on the black arcs. Further work on this piece is useless, as there is no way to remove the high point. Practice in flaking will reduce this difficulty, because, with practice, flakes will become longer and more controlled.

Broken Piece. If the piece which is being worked is poorly supported and too much pressure is applied on the top of the piece, there is danger that it may be cracked squarely across (Plate XXX, *a*, g). This danger increases as the work progresses. Accidents of this kind can be prevented only by care and patience in using the flaking tool.

Field Artillery School
Fort Sill, Oklahoma
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EARLY WATER SYSTEMS AT GRAN QUIVIRA NATIONAL MONUMENT*

JOSEPH H. TOULOUSE, JR.

INTRODUCTION

FROM THE TIME early in the nineteenth century when the ruins now known as Gran Quivira National Monument were rediscovered, speculation as to the source of the water supply has been rife. Many are the theories to account for the presence of this pueblo ruin in the midst of an expansive waterless region. These theories range from an assumption of the former existence of springs, now long dried up, to tales of marvelous aqueducts which brought water from a spring in the Gallinas Mountains to the east, a distance of fifteen miles. Recently, researches have brought to light new data in explanation of this apparent condition. It is, therefore, the purpose of this paper to assemble all of the available evidence, to evaluate and present it.

The pueblo ruin of Gran Quivira, formerly known during Spanish times as *Humanas*, lies on the Chupadera Mesa in the geographical center of New Mexico. It is one of the most extensive ruins on the mesa. The surrounding country is rolling, composed of low ridges separated by broad basin-like flats. There is no drainage from the area, and all run-off flows into intermittent lakes. These lakes are noticeable during periods of extreme rainfall and sometimes last for a year or so, but they are often dry for extended periods.

According to the Thornthwaite classification of climates,[1] Gran Quivira is in a Steppe, semi-arid, microthermal region deficient in precipitation at all seasons (DC'd). Precipitation occurs principally in the late summer (July-September), and the average rainfall ranges between 10 and 15 inches. At present, the "flats" surrounding the ruins are devoted to dry-farming. Products are mainly pinto beans and stock feed, and there is some grazing of livestock.

In the neighborhood, today, reliance is placed upon subsurface water tapped by shallow wells, from sixty to one hundred feet in depth, and upon the collection of rain water in cisterns. For stock purposes, deep wells have been drilled—this water, however, is not suitable for domestic use. At best, the water supply of the Chupadera Mesa is meager, and during extremely dry years it is necessary to haul water. Modern transportation facilities make it possible to maintain permanent residence under such conditions, but the Indian, with the primitive

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[1] Thornthwaite, 1931.

means of transport available to him, was likely to be driven away by a prolonged drought.

The closest water available to Gran Quivira, at present, is at the Montezuma ruins, five or six miles to the west, and there are other springs in the Gallinas Mountains to the east. Except for these, there is no permanent water on the Chupadera Mesa.

Two sources of data relative to the water problem of Gran Quivira National Monument are available: historical documentary evidence and archeological evidence.

HISTORICAL SOURCES

Of the several sixteenth-century Spanish exploration parties, it would appear that only the one led by Espejo actually visited the villages of the Chupadera Mesa[2]—and that, only if we accept the identification of these villages with those of the "Maguas" referred to in the accounts of the Espejo expedition. It is significant that Espejo notes that there are " . . . no running arroyos or springs. . . . "[3] The fact that the identification of these villages with those of the "Maguas" hinges upon this statement and one from Luxán's journal and is fully discussed in the report cited.

To the later Spanish settlement and the Church-State controversy, we are indebted for a more complete description of the water facilities at Gran Quivira, or Humanas.

> [. . . Concerning the pueblo of Humanos]. . . . Although it is true that this declarant gathered [the people of] the pueblo together, he only did so in order to learn from the captains what persons there were who were maintaining liaisons, that he might punish them, as is the practice in that kingdom, and in order to choose certain sheperds to drive the stock belonging to Father Fray Diego de Santander to the pueblo of Abó, for the said father had turned into that pueblo 700 sheep, 20 oxen, and 20 or 30 horses, although it has never been possible to have herds in that pueblo because there is no water. The scant water supply is in a few wells a quarter of a league from the place and forty or fifty *estados* deep. Hence it is expensive to obtain water and necessitates great labor by the Indians; they exhaust the wells, and then the people lack water. Indeed the lack of water is so acute that they are accustomed to preserve their urine to moisten the earth to make walls. . . .[4]

From this statement it is evident that shallow wells were used at Humanas, and it is of interest that present-day farmers and ranchers in the immediate vicinity of the monument report discovering pottery at depths of 20 to 40 feet during the digging of shallow wells. As these wells are dug in places where water is most likely to be encountered, it is more than probable that they are in most cases, in the same locations as the Indian's.

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[2] Toulouse, N. D. [3] Bolton, 1916, p. 180. [4] Hackett, 1937, p. 142.

With the abandonment of the Salinas province sometime between 1670 and 1675, our contemporary documentary sources vanish, and it is from secondary sources that our remaining documentary evidence is available. These latter sources contain hearsay evidence and, in some cases, personal observation; of these two, the latter is often accurate.

One of the first of these secondary accounts is that of Gregg, whose *Commerce of the Prairies* was first published in 1844. Gregg wrote a good deal concerning New Mexico; much of what he wrote was based on personal observation, but some was obviously based on hearsay. His account of Gran Quivira may belong in the latter category, since it is not certain that he visited the ruin. His account, as given below, would tend to show that he had not. Gregg says of Gran Quivira:

> Among these ancient ruins the most remarkable are those of *La Gran Quivira*, about 100 miles southward from Santa Fe. This appears to have been a considerable city, larger and richer by far than the present capitol of New Mexico has ever been. Many walls, particularly those of churches, still stand erect amid the desolation that surrounds them, as if their sacredness had been a shield against which Time dealt his blows in vain. The style of architecture is altogether superior to anything at present to be found north of Chihuahua—being of hewn stone, a building material wholly unused in New Mexico. What is more extra-ordinary still, is that there is no water within less than some ten miles of the ruins; yet we find several stone cisterns, and remains of aqueducts eight or ten miles in length [*sic*] leading from the neighboring mountains, from whence water was no doubt conveyed. . . .[5]

As later eye-witness accounts indicate, this statement that the remains of aqueducts existed should be heavily discounted—no traces of such ruins were observed by later visitors. Then too, the country intervening between the Gallinas Mountains and Gran Quivira is considerably lower. It is quite possible that Gregg was relying upon Benjamin David Wilson's account of his visit to the ruined pueblo.

Wilson visited the ruin in the winter of 1835–1836 and has the following to say:

> Myself, and expedition, six men all told, once found ourselves absolutely without anything to eat, the only result to us was great weakness. On the evening of the sixth day getting off our mules, we felt so weak that we became very much alarmed about our condition, so I had no other recourse but to shoot my faithful mule, that I had ridden over a thousand miles, it being the only animal that showed any flesh. I feel sorry about that mule yet, the killing of which occurred some forty-two years ago. On our return as before mentioned to intercept the Mission caravan, after crossing the Del Norte, at the head of *Jornada*, going eastward to the River Pecos, we had the misfortune to find no water till the fifth day at night. On the fourth day, crossing an arid sandy plain leading North to South between two parallel mountains, we saw to the North of us in the midst of this plain a large building, which encouraged us to believe that our water trouble was at an end; we went to the building and found it to be a large Church. On the northern side

[5] Gregg, 1933, p. 105.

of the building saw evidence that there had been on that site a very large town, the Church itself was built of stone, and stood almost in a perfect state of preservation, while all the other buildings had decayed. We spent the whole day looking for water without success. Just at night I discovered on the eastern side what satisfied me were remnants of a concrete aqueduct [*sic*]. Camped there that night, next morning endeavored to trace the aqueduct, which led easterly to a mountain range [*sic*]. Spent the whole day tracing it, to ascertain which was the gorge it entered, believing we should find water there. Our hopes were gratified, and our terrific sufferings ended. . . . After my return to Santa Fe narrated our discovery of that building, and some enthusiastic men went in search of it, they called it the Gran Quivira . . . [6]

The traces of the "aqueduct" mentioned in both Gregg's and Wilson's accounts appear, in the light of recent research, to have been nothing more nor less than the ditch which runs along the top of the ridge on which the ruins lie. This ditch connects with a "tank" some 200 yards east of the ruins and runs westward to another "tank" some thirty feet from the eastern boundary of the village. The water was used to irrigate a few fields on the south slope of the ridge (Fig. 28).

In the winter of 1853 a thorough examination of Gran Quivira was made by a troop of United States soldiers under the command of Major General James Henry Carleton, whose observations are quite reliable. Carleton reports:

We saw some deep pits, which were circular, and walled around like wells; we believed them to be remains of cisterns—they were not deep enough for wells; some have concluded that they were *estufas*. Two hundred and ninety feet north of the cathedral there are evident traces of an *estanque;* this, as well as the cisterns, was probably made to collect the rain water which ran from the different buildings.

Toward the east we saw a well defined road, which kept to the ridge for a hundred yards, and then turned toward the southeast, where all further vestiges of it are lost in the sand. Where it is most plainly marked along the summit of the ridge some very large cedar trees are growing directly in the middle of it; these trees look to be very old indeed.

. . . There is no sign that the ground in the vicinity has ever been cultivated, and no mark whatever of irrigating ditches. Indeed, an acequia, or open aqueduct, could not, it is believed, have brought water to the Gran Quivira, for the point occupied by the town appears to be considerably higher than the surrounding country.

We were informed by men at Manzana who had been *pastores* in their youth, and had herded sheep in this region of country, that there is a fine bold spring of water at the base of the Sierra de las Gallinas, about fifteen miles from the ruins, and that they had heard that water once ran in an aqueduct from that spring to the Gran Quivira. This could hardly have been possible, unless the aqueduct was a closed pipe; because, from appearances, the country intervening between those two points is considerably lower than either of them.

We saw no indications that there had ever been such an aqueduct, nor did we see any signs that wells had been digged in the neighborhood. From every feature of the country, both within the surrounding sand-hills, we could but be lost in conjecture as to the method adopted by the inhabitants to obtain even water to drink, let alone for

[6] Woodward, 1934, pp. 95–96.

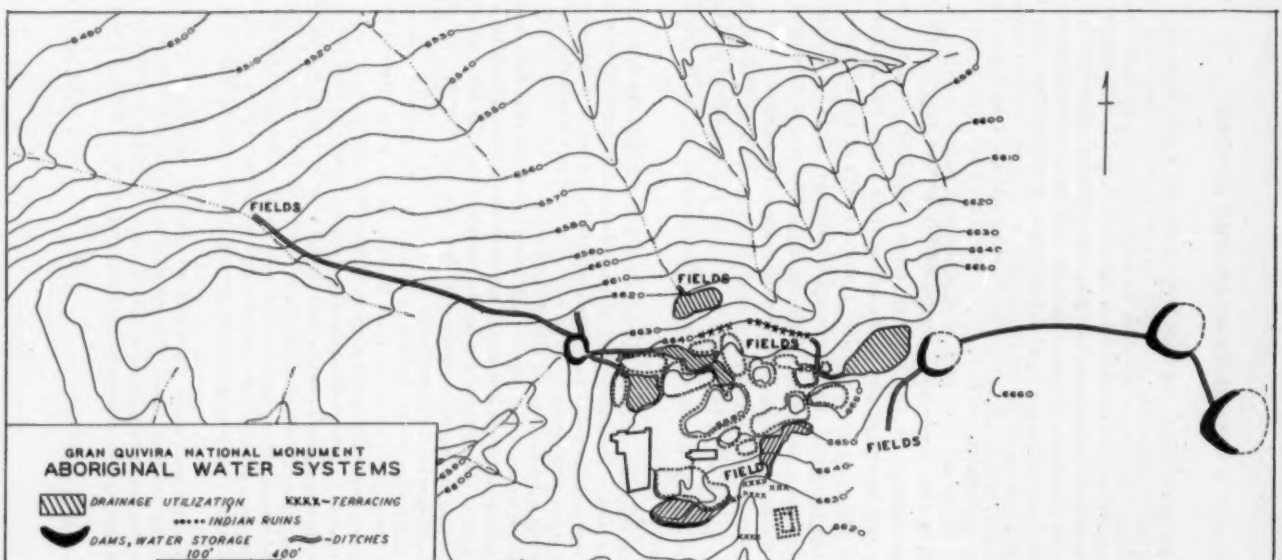

FIG. 28.—Plan of the aboriginal water systems at Gran Quivira National Monument, as determined in 1943 by the author.

purposes of irrigation, unless they were supplied by some spring or stream that has long since disappeared. The nearest point where water can always be obtained now, is the spring which the *pastores* spoke of as being at the base of the Sierra de las Gallinas, fifteen miles away. The Laguna de la Puerta is 14 miles 773 yards from Gran Quivira, in nearly a direct line; but this is said to become entirely dry in seasons of great drought.[7]

Carleton is to be commended for his observations and logic concerning the "aqueduct," but he overlooked the possibility of his "road" being a ditch belonging to a water system. It remained to Bandelier to

FIG. 29.—Plan of water systems, Gran Quivira National Monument, after A. F. Bandelier.

recognize the true state of affairs and to map the system in some detail (Fig. 29). Bandelier was wrong in a few details. He, however, described the water system at considerable length:

After examining the two churches, I turned my attention to the question of the water supply. The most diligent search revealed no trace of springs in the neighborhood, yet there appeared in the middle of the narrow street formed by the principal buildings of the pueblo a groove not unlikely a channel. Following this channel in the direction of the northeast, that is up the crest of the eminence, I noticed that it was in places from two to three meters wide (6½ to 9⅞ ft.) and about 0.50 m. (20 in.) deep. Potsherds lined its course. Three hundred meters (980 feet) northeast of the most easterly house of the pueblo the ditch terminated in an artificial pond thirty-five meters in diameter (115 feet).

A short distance southwest of this pond I found another, thirty meters (98 feet) in diameter and nearly three meters (9 feet) deep. Fifty meters to the eastward of the first was a third reservoir, forty meters (130 feet) across and two meters deep. It stood on the highest point of the ridge and still shows traces of a rim of stones. In several places this rim is broken through by gullies. The fall from this upper-most tank to the first house of the village is ten meters (32½ feet) in distance of a quarter of a mile. The rims

[7] Carleton, 1855, pp. 308–309.

of all three tanks and of the channel were covered with fragments of pottery, showing that much water had been carried from them to the pueblo, also that washing and cleansing had been performed along the channel and at the ponds.

This system of reservoirs, so arranged that the highest one emptied into the others, fully explained the mystery of the water supply for the Gran Quivira. From the lowest tank the water was led not only to the pueblo, but through it, to the western slope of the ridge on which the village stood. I followed this channel back, and found it emptied at its lower end into a fourth artificial pond constructed about thirty-five meters west of the northwestern corner of the pueblo, on the declivity, some distance below the church and northwest of it. This last reservoir is as wide as the largest of the upper ones. Its depth is still three meters, and the rim of stones around it is perfect, from northeast by north to west. In the direction of the north and northeast, two artificial channels run from it down the slope, by means of which the small garden plots could be irrigated. . . .

. . . About three miles south-southwest of it [Quivira], at the other end of a level covered with splendid grass, rises the Loma Pelada, a hill thirty-five meters (98 feet) above the surrounding plain. This bald eminence bears the remains of a pueblo similar to that of Quivira, but considerably smaller and much more decayed. The mounds are shapeless, flat; and, instead of being in long rows, are disposed in a circle around the top of the hill. I noticed two estufas, and only nineteen meters southwest from the village an artificial pond twenty-two meters in diameter. About one mile farther, at Lagunitas, is another pueblo ruin with an artificial water reservoir. It seems, therefore, that all over this arid region the villages relied upon such contrivances in the same manner as they do today at Acoma.[8]

Bandelier's error concerning the water system at Gran Quivira can best be seen by comparing his ground plan with the detailed map prepared by this writer (Fig. 28). Bandelier shows that two separate water systems were connected, whereas the intervening topography would not have permitted such a connection. There is a difference in height between the two systems which would have made it manifestly impossible for the upper tanks to have been connected with the one to the northwest of the Mission building.

THEORIES

Several theories which have been advanced by various writers concerning the water situation at Gran Quivira are relevant to this report, since at times they arise in questions by visitors to the area. One of the most prevalent is that formerly there was a mighty river which flowed through the country and furnished water to the early puebloans. Failure of this water supply is attributed to various causes, but most often to a catastrophe; i.e., it is said that the lava flow near Carrizozo, some fifty miles to the south, caused the stream to dry up. No explanation is given of the manner in which this flow caused the stream to dry up at its source, which is considered to have been on the Chupadera mesa. Some thoughts on this subject are given by U. S. Deputy Surveyor Williston,

[8] Bandelier, 1892, pp. 286–288.

whose report is on file at the U. S. Surveyor General's Office in Santa Fe. Williston, who considers several of the possible sources of water for Gran Quivira, writes:

> That water was brought there [Gran Quivira] from some distant point—and distant it would have been—cannot be the case, as the face of the country would have required the construction of numerous aqueducts for its conveyance, remains of this would be found at the present time; and why would a poeple bring water a long distance for the purpose of working lands no more valuable than such as could have been had at the water?
>
> Where, then, did the inhabitants get the water necessary for their subsistence? There are two arroyos between the ruins and the Mesa Jumanas, within a mile of the town, having well-defined water courses, which might have contained permanent water at the time that the town was inhabited. Even at the present time, the drainage from these arroyos furnishes water for a laguna some five miles below that lasts during about one-half the year. Again, springs may have existed around the rise upon which the town is situated, that, from natural causes, have become dry.
>
> The phenomenon of the failures of water is no uncommon one in this region, as is evidenced by the numerous vents where the surrounding rocks show the action of running water.
>
> A case directly supporting the assumption of the failure of the water is furnished at a place about thirty-five miles northerly from the Gran Quivira, known as "La Cienega." At this point a stream of water, furnished by two springs, and running to a distance of about a mile at all seasons of the year, which has never been known to be dry within the memory of the oldest inhabitant, has, within the last year, entirely disappeared, and even digging to a considerable depth in the bed of the late spring fails to find the stream, or the channel by which it has so mysteriously disappeared.
>
> To those at all familiar with the cretaceous formation of the southeastern portion of New Mexico, and who have seen the numerous rivers that flow hundreds of inches of water within a few yards of where they make their first appearance, and the total disappearance of these steams within a few miles, who have seen the water flowing in caves and subterraneous streams, and the fact that the whole country is cavernous, can easily imagine the possibility of a stream acting upon its cretaceous bed, and eventually wearing a channel, to connect with some immense cavern, and disappearing at once from the surface beyond all reach of human power.
>
> To the south of the Gran Quivira, at a distance of about twenty miles [*sic*] commences a *mal pais*, an immense bed of lava, sixty miles in length from north to south, and covering an area of five hundred square miles. To the southwest of this commences a salt marsh, which has an area of fifty square miles, and which is fed entirely by subterranean streams from the Sacramento and White Mountains, receiving without doubt by the same means the drainage of this plain for a hundred miles to the north. The above facts are, I think, sufficient to account for the absence of water at the present time near Gran Quivira.[9]

One refutation of Williston's theory that there were springs which have now ceased flowing is the present day occurrence of springs at Montezuma ruin and along the Chupadero arroyo, although this cannot be held as conclusive. Altogether, Williston's report shows that he gave

[9] Bandelier, 1883, pp. 31–32.

considerable thought to the problem of water at Gran Quivira. At any rate, the early reports of aqueducts seem to be entirely in error.

In addition, a close examination and survey of the region surrounding Gran Quivira will easily dispel any theory of a permanent stream of water, regardless of the reason given for its disappearance. It is noteworthy that a small intermittent lake, just a few hundred yards to the southwest of the ruins, has been overlooked by the various visitors, perhaps because it was dry at the time of the several visits. In addition, quite an extensive series of intermittent lakes occurs just a mile or so south and southwest of Pueblo Pardo ruins, which Bandelier describes as those on Loma Pelada. These lakes still accumulate water during wet years.

PRESENT EVIDENCE

The discovery of water structures at Gran Quivira comes rather as a surprise, since no permanent water is found in the vicinity. Examination reveals that the ditches, reservoirs, and terraces surrounding the ruins all served to extend a rather meager supply of water. Their construction may have meant the difference between extinction and survival. It is definitely known that abandonment of the region, and of Gran Quivira in particular was the result, in no small part, of the drought which occurred between 1660 and 1672,[10] and, in part, of the depredations of the Apaches, who were stimulated to raiding as a means of survival, since the drought seriously affected their food supply.

The remains of all four types of structures which have been defined at Gran Quivira National Monument can easily be seen there. These structures are ditches, reservoirs, terraces, and artificial drainage basins. The ditches seem to be common and vary in length from fifty feet up to several hundred yards.

Reservoirs

Five of these features close to the village have been mapped. Four are found along the ridge to the east of the ruins (Fig. 28). The most distant one, which is not shown on the map, lies some 100 feet to the east of the easternmost pair shown. This structure is quite large and is the deepest of the series. Water collected in it from a large natural drainage basin along the higher part of the ridge. A short ditch leads off the northern perimeter into a narrow, rock-lined, natural arroyo which runs toward the flats bordering the north side of the ridge. It would seem that this water irrigated a number of small agricultural plots in the neighborhood.

The remaining three reservoirs on the eastern side constitute one

[10] Hackett, 1937, pp. 272, 298.

development. The upper two are connected with each other and are, in turn, connected by a ditch along the ridge to another at the head of a small arroyo draining towards the southwest. Water was conveyed by ditch from this latter structure to a number of small plots immediately adjacent to the ruins and along the above-mentioned arroyo.

The last of the five reservoirs is located just off the northwest corner of the large Mission building. It collected runoff from the large adjacent plaza and presumably from the roofs of the surrounding buildings. Two ditches extend from it, a short one to a number of small fields due north, and a longer one toward a terraced field approximately 300 yards to the northwest.

Terraces

Several series of artificially constructed terraces are located along the north and south slopes of the ridge. The largest, on the north side, has been built up by the accumulation of considerable amounts of refuse over a long period of time. It is apparent, from the diversion of water, that crops were at one time planted upon this terrace. On the south slope, a number of small terraces can be delineated. The structures on both slopes are watered by artificially built drainage basins. These basins were not damned as reservoirs, but appear to have been utilized principally to concentrate the meager rainfall upon small areas of intensive cultivation. The terraces were constructed to conserve this water. Judging from the location of the drainage basins, rain water must have been collected in part from the roofs of the neighboring buildings.

DATING

After several efforts had been made to distinguish, by cross sections and other means, between periods when the several water structures might have been built, it was determined that no definite period could be assigned for their construction. This was due principally to the lack of datable sherd material. Consequently it is impossible, at present, to assign these works to either the historic or prehistoric periods. A present-day check of the remains described by Bandelier at other ruins in the vicinity fails to reveal such structures. These may have so disintegrated since Bandelier's visit as to be wholly unrecognizable today.

It is noteworthy that prehistoric water systems occur also in the Southwest at Mesa Verde,[11] at Aztec,[12] in Chaco Canyon,[13] in Galisteo Basin,[14] and in southern Arizona.[15] Nevertheless, the possibility that the

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[11] Stewart, 1940, pp. 201–220.
[12] *Ibid.*, pp. 329–340.
[13] Hodge, 1912, pp. 620–621.
[14] Nelson, 1914.
[15] Turney, 1929; Hodge, 1912, pp. 620–621.

Franciscans inspired construction of such a system here, or even the possibility that they modified and improved upon a then-existing system should not be ruled out.

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Gran Quivira National Monument
December, 1943

SALT EROSION

JULIAN D. HAYDEN

SEVERE, occasionally disastrous, erosion of the bases of adobe walls in the arid and semiarid regions of the Southwest is a phenomenon which has long been observed and commented upon. In historic times, erosion of this type has caused collapse of adobe buildings in the Rio Grande Valley of New Mexico; about Tucson, Arizona, it is serious, as it is in the Salt River Valley. Evidence of similar erosion has been noticed in excavations of prehistoric massive adobe or caliche walls of Hohokam and Salado structures of the thirteenth and fourteenth centuries A.D. in the Salt and Gila River valleys of Arizona. Repair to check erosion and prevent collapse of walls was the primary purpose of Cosmos Mindeleff's stay at Casa Grande in the 1890's. Fewkes noticed similar cutting of standing walls at the nearby Adamsville site.

Both Fewkes and Mindeleff present excellent photographs of the work of this erosion in their reports on the Casa Grande, and both speculated upon its cause.[1] Fewkes remarked upon "deep erosion of walls from former weathering," noted that the greater erosion of walls was on the east side, and ascribed this to the rain beating on that side. Mindeleff attributed this erosion to "sapping," in which driving sand cuts away wall bases after they have been moistened by rain. Since, however, southwest winds prevail at Casa Grande, and since erosion occurs with equal severity on interior walls at Casa Grande and at all other eroded sites, neither of these hypotheses apears to be of value.

During the excavation of the great Hohokam-Salado mound of Pueblo Grande, near Phoenix, Arizona, by the author for the National Park Service, 1936–1940, it became evident that such erosion during prehistoric times had become so severe as to have played an important part in the history of the site. It was a problem of some interest, therefore, to attempt to ascertain the causes of this erosion, with the hope of securing information which would aid in reconstructing the history of the village. Attention was turned to the Pueblo Grande Laboratory, a modern adobe-walled building without foundations, adjacent to the mound under excavation. The bases of its modern walls have been very severely eroded (Pl. XXXI, *a*), and the inception, course, and final checking of the cutting were carefully observed by the author, with interesting and definite results.

[1] Jesse Walter Fewkes, "Casa Grande, Arizona." Bureau of American Ethnology, *28th Annual Report* (1912), Pls. 2, 10, 18, and pp. 92, 105, 116. Cosmos Mindeleff, "Casa Grande Ruin," Bureau of American Ethnology, *20th Annual Report* (1896), Pls. 54, 55, and p. 300.

Erosion of the type discussed in this report results in a troughing of the wall which has a characteristic profile in section. Where the wall is of adobe or caliche-silt mixture it is cut deeply below a well-defined line, generally 15–20 centimeters above the ground surface, and the trough thus formed overhangs and is flat-topped in section. A talus of disintegrated adobe, which is soft, fluffy, and without laminations, increases in depth as the trough deepens, and the upper limit of erosion rises as the talus accumulates. In pure puddled caliche, the trough overhang is not always horizontal in section, but may rise toward the wall face at an appreciable angle. This erosion results in a section different from that caused by rain-washing, in which the wall is cut most deeply at its junction with the ground and gently approaches the plane of its original face at a varying distance from its base. It is usually streaked with mud rivulets.

Samples were taken from the walls at Pueblo Grande Laboratory, where, in the damp situation of the pump house, a wall one foot thick had been cut entirely through in five years. These samples were analyzed for soluble salt content.[2] The interior of the upper wall contained 2,810 parts per million of soluble salts, whereas the scrapings from the overhang of the erosion trough contained 87,590 parts per million of soluble salts. Of this total, the hygroscopic salts, the chloride, carbonate, and nitrate of calcium and magnesium comprised in all 63,120 parts, nitrate alone amounting to 32,550 parts per million. Similar results were obtained in the great mound, as shown below:

		Ks*
A—West face east wall Room 60, compound	wall base	4100
	wall top	132
B—North wall mound, 12′ high, of flat stone slabs in adobe, erosion over all	wall base	3200
	wall top	2570**
C—Wall upon north end mound, base 12′ above ground level	wall base	2200
	wall top	84
D—Southeast corner of mound, outside	wall base	4900
	wall top	107
E—Southeast room of mound, inside of wall 10′ high	wall base	1680
	wall top	52
F—Damp ground near mound, with salt efflorescence on surface		1870
Pumphouse wall, for comparison in Ks	wall base	4180
	wall top	278

* Ks is specific electrical conductance at 25 degrees Centigrade, at soil saturation.

** The north wall was backed to its top by mound fill; thus the flow of moisture to the exposed wall face was the same from its base to its top.

[2] Tests by Paul Brown, soil chemist for the Salt River Valley Water Users Association.

The results of these tests indicated a hypothesis which might plausibly explain erosion of wall bases. Briefly, soil moisture, whether derived from rain water or from a high-standing water table, rises in adobe walls to that point at which it is checked by evaporation. Soluble salts in this rising moisture are thus carried upward and deposited at the evaporation line on the wall face, generally about 15–20 centimeters above the surface of the ground. Here this increasing concentration of salts acts in two ways to destroy the character of the adobe. Crystallization mechanically shatters the adobe, causing continual shedding of adobe particles at the surface. Also, concentration of salts above an unknown limit of tolerance results in chemical destruction of the cohesive properties of the material and in its further disintegration (a fact well known by adobe workers, who do not use adobe soils from "alkali"-bearing areas for adobe bricks, and who dislike to use water with a strong "salt" content for mixing adobe). As this concentration increases, the salts, being hygroscopic agents, attract moisture from the air, especially on cloudy days and during the nights, and the process of erosion is accelerated and may proceed with rapidity. Moisture may often be observed in the troughs, especially in the mornings.

Since this erosion of wall bases may, then, be accepted as being directly dependent upon the presence or absence of soluble salts in the soil, as well as upon soil moisture, the action has been termed "salt erosion," with the approval of the late T. A. Hayden, Assistant Chief Engineer of the Salt River Valley Water Users Association.

Results obtained at Pueblo Grande were corroborated at the University Indian Ruin, near Tucson, Arizona. This massive-walled structure, situated upon a high, well-drained terrace, exhibited little trace of salt erosion upon excavation. The mound fill was almost bone-dry and was dusty to its base. In the year following excavation, however, rainfall records for fifty years were broken, the mound fill was saturated throughout its mass, and the walls, except for the most solid, were likewise soaked. The results were striking. Salt erosion began on all exposed faces at the evaporation line and cut very deeply (Pl. XXXI, *c*). In a wall which was smoothly plastered and hard when exposed, troughing took place to a depth of 10 centimeters (Fig. 30, *c*); the trough overhang (accidentally mixed with other soil during sampling) contained 7,500 parts per million of soluble salts, while the wall interior above contained 840 parts.[3] In a wall which had been seriously cut by salt erosion prior to the erection of an abutting wall, troughing was resumed, and analysis showed 36,000 parts of salt in the trough and 950 parts in the interior

[3] Analyses by the College of Agriculture, University of Arizona.

of the wall near the top (Fig. 30, *d*). In another wall, only one face of which had been exposed, troughing took place, with 36,000 parts of salt in the trough and 530 parts in the wall top.

Thus, an exceptionally wet season caused very serious erosion of walls. Had the rooms still been occupied, this might have been the cause of collapse of walls and would certainly have necessitated attempted re-

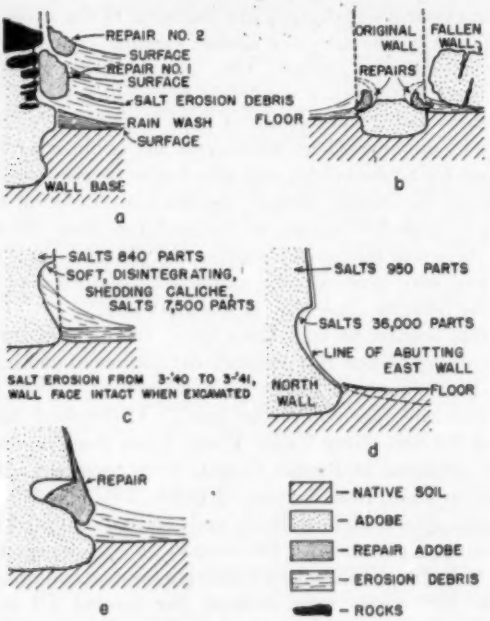
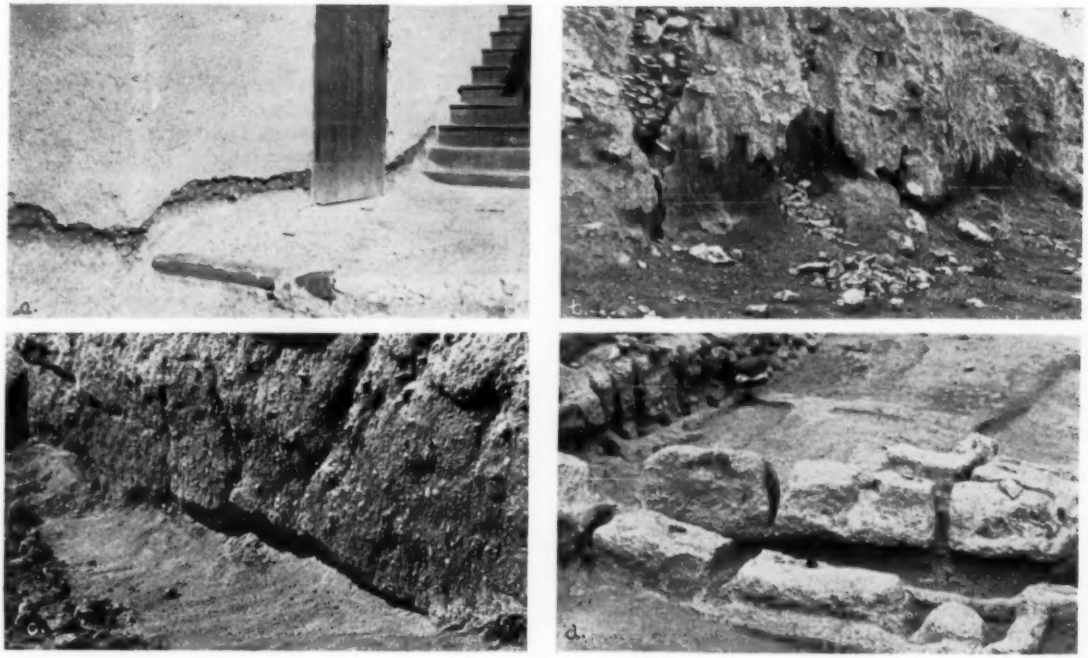


FIG. 30.—Examples of wall sections showing the effects of salt erosion at the base level and subsequent attempts at repairing the damage. *a*, *b*, from Pueblo Grande near Phoenix; *c*, *e*, from the University Indian Ruin near Tucson.

pair of erosion troughs. That such a rainy season had occurred during occupation of the mound was shown in one room in particular, where extensive repairs had been accomplished (Fig. 30, *e*).

The archaeological value of these observations was made clear at this site, for, through a fortunate combination of stratigraphy and other factors, erosion during occupation of the mound was definitely associated with erosion by rain-washing in the adjacent, low-lying village, and was assigned with some certainty to the same year.



Salt erosion. *a*, Pueblo Grande Laboratory, southeast corner of east wing. Note rise of erosion trough along cement steps at foot of stairway, and damp area just above it at that point; *b*, Pueblo Grande mound, north wall. Fill behind wall to top stores moisture and feeds it to face. Note cutting under overhanging slabs, dark areas of moisture, and talus of salt erosion debris; *c*, University Indian Ruin. Trough cut between April, 1941, and March, 1942, with talus removed to show depth of trough; *d*, Pueblo Grande, showing collapsed south wall of room in northwest corner of Salado compound. Section through base of fallen wall is shown in Figure 30, *b*.

At Pueblo Grande, study of salt erosion threw valuable light upon the history of the site, exploding a widely disseminated theory as to the origin of such artificial mounds, with their earth- and rubbish-filled massive-walled enclosures, which held that such filling had been done in successive vain attempts to escape rising ground water and attendant soaking of floors. The massive retaining walls of the nuclear mound were erected during the Soho phase by the Hohokam, and Hohokam-type houses were built upon the mound. The wall of this mound showed little or no evidence of salt erosion, indicating that the water table at that period was low, and that waterlogging of the valley had not occurred. However, walls of several later Salado (Civano phase) structures which were built upon the destroyed Hohokam houses, on and all about the mound, showed increasingly serious erosion. During the latter part of the Salado occupation, walls within the compound enclosing the mound fell because of such erosion, and their debris was but partially removed by the Salado people. In Plate XXXI, *d*, the adobe rim about the stump is the remnant of a filling of caliche placed in the salt erosion trough in an attempt to check erosion and strengthen the wall. The wall reinforced with posts, which is shown in the upper left, and which was built against the troughed wall to strengthen it, was itself deeply troughed about the posts. Figure 30, *b* shows a section through the base of the fallen wall.

Other walls, notably the massive north compound wall, two stories high, were reinforced after erosion with heavy posts leaned against them and encased in caliche; this caliche in turn was eroded. Repairs of erosion troughs, done vainly by packing the troughs with caliche, were successive toward the last of the occupation, each repair base resting upon salt erosion debris of the earlier repair (Fig 30, *a*)

A probably valid conclusion can be drawn from these observations: that continued and intensified canal irrigation of the Salt River Valley by the Salado people raised the water table, increasing more and more the moisture content of the soil, bringing about a concentration of salts at the surface and very rapidly increasing erosion of wall bases. This, then, as demonstrated at Pueblo Grande, would appear to have been a development with considerable nuisance value incidental to the fundamentally important and insoluble problem of water-logging, which may be presumed to have been one of the important causes of the abandonment of the Salt River Valley by the Salado near the end of the fourteenth century. In historic times, water-logging of the valley by white farmers, in some forty years of increasingly intensive irrigation, rendered necessary the installation of a great system of drainage pumps and ditches to make possible the continuation of farming activity.

Thus, the value of this apparently minor phenomenon becomes obvious. From the situation of the salt erosion, the time relationship of adjoining walls may be deduced and, in certain cases, even the relative chronology of different sections of a site. The disintegration of walls makes clear the origin of certain otherwise puzzling sediments, which are soft and homogeneous in character and slightly or not at all laminated. It may also be possible to determine the occurrence of especially wet seasons (as at University Indian Ruin). More important, the study of salt erosion in a site and in an area may enable the field worker to postulate major developments in the history of an entire valley.

Yuma, Arizona
January, 1944

WILLIAM HENRY CAMPBELL, 1895–1944

ERNST ANTEVS

WILLIAM H. CAMPBELL, Fellow and Trustee of The Southwest Museum in Los Angeles, drowned in Lake Tahoe at Glenbrook, Nevada, June 3, 1944, when he returned from fishing in the beautiful, still summer evening. Nobody witnessed the accident, but he apparently stumbled as he approached the mooring post, was knocked unconscious, and fell overboard. He is survived by his wife, a brother, a sister, and nephews.

William Campbell was born in Los Angeles, California, October 28, 1895, the son of Peter Craig Campbell of Kirriemuir, Scotland, and Anna Henry Campbell of Reading, Pennsylvania. He was brought up in Los Angeles, where he attended public schools. In the spring of 1917 he enlisted for war service. After a year's training at Camp Crane, Allentown, Pennsylvania, he was sent overseas in June 1918. He served as an ambulance driver on the Italian front and received the War Cross for bravery in action. Just prior to the armistice he was severely burned by mustard gas. He returned home in the spring of 1919.

On May 27, 1920, Campbell married Elizabeth Warder Crozer of Upland, Pennsylvania. For a time he worked with the Emergency Fleet Corporation at Wilmington, Delaware; but, in February 1921, on account of ill health caused by gas burns in the lungs, he moved to Pasadena, California. Gradually his health broke down completely. In the hope that dry desert air would bring relief from his lung ailment, Mr. and Mrs. Campbell went in December, 1924, to the desert 150 miles east of Los Angeles, pitching their tent at the oasis of Twentynine Palms. Here they had only two neighbors within many miles. In February, 1925, Campbell homesteaded a tract of land near the oasis, where they built a cottage which formed the nucleus for their beautiful home.

With their home built and time at their disposal, the Campbells looked about for some intellectual pursuit. They were located on the southwestern side of the Mohave Desert, the vast triangular region which, with its apex 60 miles north of Los Angeles, spreads its base on the Colorado River from Yuma to Boulder Dam. The collection and study of Indian artifacts which they had observed at water holes and in caves seemed an interesting and worthwhile pastime. The Indians themselves had departed from the region in 1913, but they were brought back to life in the narratives of Mr. William McHaney, one of the Campbells original desert neighbors. Mr. McHaney, a prospector who had traversed the region with his burros since 1879, had lived, hunted, and traveled with the Indians and had acquired an intimate first-hand

knowledge of their customs and mode of living. Thus, the Campbells commenced early in 1928 to collect Indian artifacts in caves and on camp sites in the archaeologically virgin territory around Twentynine Palms. Somewhat later they made the acquaintance of Mr. Charles A. Amsden (1899–1941), then curator of archaeology at The Southwest Museum. With encouragement and advice from him, they pushed their field work and extended its periphery. In November, 1929, they deeded their entire large archaeological collection, as well as any future collections, to The Southwest Museum. The assembled material was kept for some years at Twentynine Palms, where the Campbells built a laboratory and museum named The Desert Branch—since 1936, The Twentynine Palms Laboratory—of the Southwest Museum. During 1930–1932 Mr. Edwin F. Walker acted as curator for the collection and the museum. The modern and recent archaeology of the area was described in "An Archeological Survey of the Twentynine Palms Region."[1]

While the Campbells always collaborated, there was a certain division in their labors and an individuality in their contributions. Both shared in field work and acquired great skill in locating sites and finding artifacts. In looking for sites they would cruise at low speed, scanning the ground. Mrs. Campbell did the writing and most of the laboratory work. Mr. Campbell did the mapping, determined the elevations, and stood for equipment and trasportation. He outfitted his station wagons with water and extra gas tanks, ice box, and pantry, permitting week-long trips to waterless places. He equipped them with special gears and extra wide tires for driving across soft sand and silt. He possessed the mechanical knowledge and ability to mend the cars when they broke down. Most important, he acquired such a skill in desert driving that there was hardly a place he could not reach.

Having heard that fossil bones and worked flints had been seen on the main wash of the Pinto Basin, fifty miles southeast of Twentynine Palms, the Campbells themsevles located old camps strewn with bones and artifacts, then called upon expert help from the Geology Department of the California Institute of Technology. Association of the artifacts and the bones of extinct camel and horse could not be proved but appeared probable, and the geology and the location of the camps suggested that these dated from an age with much heavier rainfall—from the Pluvial age. The results of the study were presented by the Campbells, David Scharf, and Charles Amsden in "The Pinto Basin Site."[2]

Now the Campbells scoured the desert far and wide "with two main

[1] *Southwest Museum Papers*, No. 7, 1931.
[2] *Southwest Museum Papers*, No. 9, 1935.

objectives: one to cover all likely areas where sites might be found, the other to try to discover if cultural groups follow certain geologic formations and if so, to place these types in their proper sequence." They found numerous modern arrow point-potsherd sites, located near present-day water holes; many modern arrow point-non potsherd camps near water holes, or sometimes fairly far from water; and many dart point-scraper-non potsherd camps on old stream terraces and abandoned shorelines far from modern water. This survey was outlined in "Archaeological Problems in the Southern California Deserts."[3] The most promising district, that at Baker, 150 miles northeast of Los Angeles, was studied in greater detail with the aid of Charles Amsden, Francis Bode, Joseph Barbieri, and Ernst Antevs and supplied one of the better evidences of Pluvial age man in North America. This investigation was published under the title "The Archeology of Pleistocene Lake Mohave."[4]

Having covered most of the Mohave Desert, the Campbells combed the contiguous parts of the Great Basin and ranged far into Nevada and Utah in their ever-widening search for records of Early Man. To be closer to this promising field, they built a home on Lake Tahoe at Glenbrook, Nevada, which they made their legal residence. One of their discoveries in Nevada is recorded in a preliminary paper, "A Folsom Complex in the Great Basin."[5] Though they have large unpublished collections and field data from the Great Basin, they hoped to devote many more years to its exploration.

In recognition of his contributions to archaeology, Campbell was appointed Fellow in Archeology of The Southwest Museum in February 1932, and was elected Trustee of the Museum in January 1938, and again in January 1941.

As one of the first homesteaders at the Twentynine Palms oasis, Campbell naturally took a very active part in the establishment of the community and in its evolution into a residential desert town. He devoted much thought and time to the planning and construction of the roads. He donated land for the original American Legion building and both land and funds for the first public school. He served as local Legion Commander in 1929, 1930, and 1933.

Bill Campbell was a modest, quiet, and good-natured man with an ever-ready friendly smile. He had a keen mathematical intellect, great resourcefulness and courage, and a calm that stood up in the most

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[3] AMERICAN ANTIQUITY, Vol. 1, April, 1936, pp. 295–300.
[4] *Southwest Museum Papers*, No. 11, 1937.
[5] *The Masterkey*, January, 1940, pp. 7–11.

exasperating situations. These sterling qualities won him many firm friends. His mental stability, together with his keenly observing eye, practical mind, and extraordinary skill in handling a car, made him an outstanding desert explorer. In devoting his talents, time, and resources to the arduous task of surveying the deserts of southern California and the Great Basin, Campbell rendered a great service to archaeology.

The Corral
Globe, Arizona
September, 1944

FACTS AND COMMENTS

ON THE ILLINOIS CONFEDERACY AND MIDDLE MISSISSIPPI CULTURE IN ILLINOIS

In a recent issue of AMERICAN ANTIQUITY,[1] Donald E. Wray and Hale Smith have offered an "entirely hypothetical and tentative reconstruction" concerning the relationship of tribes of the Illinois Confederacy to late Woodland, Middle Mississippi, and Oneota manifestations in the state of Illinois. This paper appears to be a condensed version of a somewhat more elaborate one published in the Transactions of the Illinois State Academy of Science.[2] The authors concede that their reconstruction may be "wrong in some of its assumptions and conclusions," but express the opinion that "it seems to be in accord with every type of evidence we can find. . . ."

Obviously, only an exhaustive and critical examination of all relevant evidence, including that yet to be accumulated through carefully controlled spadework, can show whether—or to what extent—Wray and Smith are on the right track. Obviously, too, no interim criticism of a working hypothesis is justified merely because the hypothesis strikes one as ingenious rather than plausible. I am not unmindful that "the picturesque prophecy of today may well be the scientific fact of tomorrow." In the present case, however, it seems to me that the "picturesque prophecy" is based on a series of assumptions, poorly reasoned inferences, and outright conjectures whose admissibility as evidence is very much open to doubt.

As the authors see it, their evidence is of two types—distributional and chronological. Concerning the first, they observe that on the basis of early French records "the area of the [Illinois] Confederacy coincides nicely with the known region of heavy Middle Mississippi occupation." Then, as an effort "toward the more exact application of the distributional approach," they note further that "several of the French accounts mention the fact that a number of Illinois villages of the Peoria tribe stood around the shores of Lake Peoria. A survey of sites in this area has shown that the majority are Middle Mississippian." I gather that from this the reader is to infer a direct connection between Middle Mississippi sites and the Peoria villages, and, further, that these Middle Mississippi-Peoria sites are of the early European contact period.

This, of course, is by no means the first such gross correlation to be attempted in American archaeology, nor is it likely to be the last. It is an acceptable device when it can be shown that direct ethnohistorical correlations with archaeological findings at specific sites are impossible but that, on the other hand, the distribution of a specific, clearly delineated archaeological complex (which "Middle Mississippi Culture" is not) coincides with the range of a known tribe or group of closely related tribes. It is an *indirect* historical approach, and acquires probability in proportion to the number of specific concurrences established. Unsupported, however, such a suggested correlation remains little more than an interesting possibility; and Wray and Smith proceed to invalidate their own argument with the statements that "the numerous Middle Mississippi sites have never yielded any trade materials" and, again, "by the time the French explorers arrived most if not all of the Illinois bands had adopted Oneota culture." If this latter remark is valid, why was the reader led to infer a Middle Mississippi-Peoria correlation on the strength of historical documents and archaeological surveys?

If the distributional evidence as presented is of questionable value, that adduced as chronological impresses me as even more tenuous. The authors seem to accept tacitly

[1] Vol. 10, No. 1 (July, 1944), pp. 23–27.
[2] Vol. 36, No. 2 (1943), pp. 82–86.

the post-De Soto dating postulated by Ford and Willey for the florescence of Middle Mississippi culture and the "Buzzard Cult"—a "key assumption," they call it, and, one may add, a rather knotty problem whose current interpretation appears to rest wholly on circumstantial evidence. Nowhere, so far as I am aware, have the supporting data been detailed for critical evaluation and independent judgment. That Middle Mississippi culture is *relatively* late seems undeniable; that it is perhaps largely post-De Soto is possible, but by no means proved. That a "Buzzard Cult"—or at any rate a material complex which might be interpreted as evidence thereof—existed, is also possible; but that it is assignable to the *circa* 1600 period and later is an assumption for which no convincing or conclusive evidence has yet been presented. What is urgently needed now is not uncritical repetition of these time estimates, but a rigorous, independent, and dispassionate testing at every point where they touch a local culture sequence. Continued tacit acceptance, whether as a convenient working hypothesis or because the estimates represent the best informed opinion or for some equally self-satisfying reason, may in the long run lead to entrenched preconceptions and a badly warped picture of a local or regional archaeological situation. Wray and Smith, it is fair to add, are by no means alone in their use of these guesses. But so long as direct dating for specific Middle Mississippi cultures or the hypothetical "Buzzard Cult" is impossible, their precise nature and duration undefined, and their assumed lateness in relation to European contact substantiated by nothing more concrete than "guess-dates," their utilization as a chronological point of reference in cross-dating geographically remote archaeological horizons seems highly dubious scientific procedure.

In their use of the Oneota data, Wray and Smith appear to have made several highly questionable inferences that do not logically follow from the observational evidence. I doubt that many field investigators directly concerned therewith are ready to agree that "most Oneota sites are contact," hence the Oneota "is a late development, presumably after 1650." It is true that European trade goods have been found at a number of Oneota sites, but they are absent from Leary, from most of the Iowa stations, and, so far as my information goes, from some if not most Wisconsin and Minnesota manifestations of Oneota. The observed facts thus indicate that Oneota is only in part a contact horizon, though from the seeming homogeneity of the remains over a wide area it has been conjectured by some, including myself, that the Oneota flourished during a relatively short period. But who can say how long the precontact period may have been? I know of no practicing archaeologist other than Bennett[2] to whom "the relative time span of Oneota is known," at least in terms of our own absolute chronology. Conceivably, the Oneota horizon may ultimately prove to postdate 1650, but on present evidence I do not believe that anyone can demonstrate objectively that its "life" span was limited to 100 or 200 or 300 years, or whatever the case may be. Since, moreover, no one has yet shown what was the life span, in terms of years, of any given Middle Mississippi complex—e.g., Spoon River, Trappist, Old Village, etc.—and since the extent and exact point of temporal overlap of Oneota and Middle Mississippi manifestations cannot yet be objectively demonstrated, I believe it is sheer folly to assume that cross-finds of Oneota sherds or other traits in another undated site-complex of unknown duration can give more than a very general correlation.

Wray and Smith have again gone far beyond their evidence when they attribute to the Quapaw an Oneota culture complex—on the premise, one gathers, that because some Oneota peoples were probably Siouan and the Quapaw were Siouan-speaking, the Quapaw must also have had an Oneota culture! So far as I can determine, no one has yet

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[2] AMERICAN ANTIQUITY, Vol. 10, No. 1, p. 20.

isolated a historic archaeological complex that on documentary grounds can be proved to be Quapaw. Where, then, is the justification for the authors' assertion that the Quapaw were qualified "on both historical and archaeological grounds" to be transmitters of an Oneota-"Buzzard Cult" trait assemblage from the south to the Illinois tribes? And, having recorded their belief that Oneota is a contact (i.e., post-1650) development, by what reasoning do the authors reach the conclusion that such an Oneota-"Buzzard Cult" assemblage should be looked for "in the many large town sites mentioned by De Soto as located along the Mississippi River below what is now Memphis [which] would date from the late sixteenth and early seventeenth centuries?"

The ambiguity that mars both published versions of the Wray-Smith hypothesis is nowhere better exemplified than in the discussion of the Crable site, which is said to offer a "key for putting in chronological order many of the cultural changes which were occurring so rapidly in protohistoric times." This site, say the authors, exhibits "a composite culture," "two distinct complexes," "an obvious blending," and "two unassimilated complexes." Just what is meant by this "blending" which left the participating complexes "distinct" and "unassimilated" is not made clear, and I strongly doubt that solution of the important problem of Middle Mississippi-Oneota relationships has been very materially speeded by Wray and Smith's remarks.

It will be interesting to see what the ethno-historians have to say regarding the validity and applicability of documentary citations introduced by Wray and Smith to bolster their hypothesis. Here I shall call attention to but one more suggestion by the authors which does not seem to accord with any fact on record. This relates to the southward withdrawal of the Illinois from Wisconsin and northern Illinois, said to have been completed by about 1650, which "may have been a reflection of the development of Plains horse culture and the shifting in tribal locations which it brought about." As Haines' study of the historical documents has shown,[4] the horse spread very slowly from the Santa Fe region during the first half of the seventeenth century, its dispersal being greatly accelerated by the Pueblo revolt of 1680. Haines further concludes that "very few horses could have been owned north of central Kansas before 1700." In the light of these findings, there seems not the slightest reason to attribute to the disturbing influence of the Plains horse any of the presumed early seventeenth century ethnic movements from Wisconsin into Illinois.

That the proponents of any interpretation or work of synthesis may go beyond established facts is a truism which I accept. At the same time, I maintain that it is the duty of the synthesists to show clearly the steps in their reasoning and, above all, to be certain that the reasoning is in accord with every type of evidence available. Where the argument does not accord with observed facts or inferences logically derived therefrom, and in reality runs counter to at least some of its facts and premises, it seems timely to ask in what way such a dubious speculative proposition, however ingenious or tentative, can really contribute to the progress of archaeology.

The strictures in the foregoing paragraphs are by no means intended as a blanket condemnation of all attempts at synthesis and interpretation of data. Archaeology, in common with other fields of learning, obviously cannot hope to progress far without venturing generalizations and attempting reconstructions based on its accumulated observational data. I am very far from agreeing with Bennett's dictum that American archaeology can be considered as, in any sense, nearing "the close of the fact-gathering period,"[5] though I do agree with those who feel that the time for general synthesis is approaching in the eastern United States and the Midwest; after nearly a century of

[4] *American Anthropologist*, Vol. 40, No. 3 (1938), p. 429.

[5] AMERICAN ANTIQUITY, Vol. 9, No. 2 (1943), pp. 208, 215.

exploration and excavation some of the broader truths should be emerging. But, while workers in other fields and those with differently oriented interests in our own field needle us for a comprehensive "historical interpretation," it must not be forgotten that the area involved is vast and complex and the available data of very uneven reliability and completeness. Any over-simplified treatment, no matter how attractive its simplicity or "logic" make it appear, may well lead to self-delusion unless relentlessly checked and cross-checked against the observational evidence.

This is not the place to argue the question whether American archaeology in its present status can be considered a science. I believe it can insofar as it represents "a body of established, verifiable, and organized data secured by controlled observation, experience, or experiment." Further, I hold that, whatever its ultimate goal, archaeology can progress surely only as its practitioners adhere to the method and attitude of science—in essence, the acceptance of observed and verifiable facts, the eschewing of unsupported speculations and personal dicta, and a circumspect tolerance of that for which the observational, experimental, or experiential evidence is not immediately at hand. I regard as most inauspicious the tendencies currently manifested by some eastern archaeologists, such as the uncritical repetition and utilization of guess-dates, the unbridled piling of assumption on assumption, the acceptance of cultural continuity in every trait similarity regardless of remoteness in space or disparity in time, the disregard for the necessity of adducing supporting evidence where archaeological complexes are ascribed to one or another "protohistoric" ethnic group, and the promulgation of so-called chronological and cultural sequences consisting primarily of culture complex labels bandied freely about without a clear definition of their meaning and content—tendencies which, I firmly believe, are not in accord with scientific procedure and are much more likely to retard than advance us along our road to a meaningful, comprehensive, and trustworthy historical reconstruction and interpretation.

WALDO R. WEDEL
U. S. National Museum,
Washington, D. C.
August, 1944

THE PEDESTAL VESSELS OF THE MADISONVILLE SITE

In his recent study of the Fort Ancient aspect, J. B. Griffin has described two unique vessels from the Madisonville site as follows:

> Two of the most unusual vessels from the site have pedestal bases. One of them was recovered relatively intact from the right side of the skull of an extended adult male. This jar is entirely typical of the Madisonville Cord-marked jars with the exception of the stand, which is an anomaly in any cultural group in the eastern United States. The vessel and the stand are 19 cm. high, and the stand alone is 6.5 cm. This unique piece is in the Cincinnati Art Museum. On the other jar the pedestal has been broken off, but there is no doubt but that such a base was formerly attached. This jar does not have any handles, but in its other traits it is a representative Madisonville Cord-marked jar.[1]

From Hooton and Willoughby we glean the additional information that these bases are solid, and that the base of a third vessel of this type was also found at the site.[2]

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[1] James B. Griffin, *The Fort Ancient Aspect* (University of Michigan Press, 1943), p. 137. The complete vessel is illustrated by Griffin in Plate LXVI, Fig. 4. The same vessel appears as Pl. CLXIII, Fig. d, in Holmes' "Aboriginal Pottery of the Eastern United States," *20th Annual Report, Bureau of American Ethnology*, 1903. Both vessels are figured in Hooton and Willoughby, "Indian Village Site and Cemetery near Madisonville, Ohio," *Papers of the Peabody Museum of American Archaeology and Ethnology*, Vol. VIII, No. 1, 1920, Pl. 24, *k, l*.

[2] Hooton and Willoughby, *op. cit.*, p. 80.

Griffin also mentions a similar vessel figured by Orr[3] and found at St. David's, Niagara district, Ontario. Its cultural context is unknown, but Orr remarks that the pottery is "purely Indian." The specific form of the Madisonville and St. David's pedestals is different, but they are more similar to one another than they are to any other forms known from the region.

What interpretation are we to place on these four unique vessels? Holmes said in discussing the Madisonville example: "The addition of this feature [the pedestal] was probably an experiment on the part of the potter, who was possibly attempting in a crude way to copy the work of his southern neighbors."[4] He was doubtless referring to certain Middle Mississippi forms, but, although Middle Mississippi influences are present at the Madisonville site, the pedestal of this vessel can hardly be considered related to the annular bases and occasional pedestal-like bases of Middle Mississippi vessels. In fact, it is difficult to see a relationship to any form in North American ceramics. Griffin is correct in considering it an anomaly. We are forced either to regard this feature as a unique independent development or to look outside the aboriginal sphere of influence for its derivation.

That aboriginal copies of European objects were made upon occasion is almost certain. Quimby[5] has shown that there is a strong possibility of such copying at the Fatherland site in Louisiana, where a teapot-shaped vessel in a native red-slipped ware was found associated with a burial accompanied by trade materials. He notes that Du Pratz requested copies of his French earthenware from the Natchez, and that the copies were "a quite beautiful red."

As has long been recognized, evidence of European contact is present at the Madisonville site in the form of trade material, the date and cultural provenance of which are, unfortunately, unknown. With the fact of contact established, however, the non-aboriginal derivation of the pedestal bases suggests itself as a distinct possibility. The one complete vessel suggests a goblet in form, and the communion chalice seems the most likely of the European goblet vessels to have been seen in the early contact period. The known distribution of the pedestal bases, at Madisonville and Niagara, strongly suggests the possibility of French influence. Even if the ascription of the influence to the French is not acceptable, because of lack of documentation, the strong possibility remains that these unique pedestal bases represent crude native copies of a European object of goblet-like form.

JOHN W. GRIFFIN
Chicago, Illinois
April, 1944

ANOTHER SUBTERRANEAN PASSAGE LEGEND

Four legends of subterranean passages in as many parts of Middle America have recently been reported or cited by J. Eric S. Thompson (*Apuntes Sobre Las Supersticiones de Los Mayas de Socotz, Honduras Britanica*, in *Los Mayas Antiguos*, Mexico, 1941). They range in time from the present back to 1580, and in space from Zapotecan to eastern Maya country. He concludes that they were general among Mexican Indians and mixed bloods and may have been carried to New Mexico by Spanish-speakers, citing Kidder as

[3] R. B. Orr in "27th Annual Archaeological Report, 1915," *Appendix to the Report of the Minister of Education, Ontario*, Toronto, 1915, p. 92.

[4] Holmes, *op. cit.*, p. 185.

[5] George I. Quimby, "The Natchezean Culture Type," AMERICAN ANTIQUITY, Vol. VII, No. 3 (1942), p. 263.

authority for existence of a local belief in a passage connecting the ruins of Pecos with Taos.

Two of the reported stories envisage a cave at one end of the passage, and in one of them the famous ruins of Palenque are at the other end. I can report another of this sort, also with Palenque as a terminus. There is a small cave on the left bank of the Usumacinta river just above the point where it enters the Tabasco plain at a point called Boca del Cerro. While I was examining it some years ago, one of the boatmen volunteered the information that some people believe a passage leads from it to Palenque. I neglected to ascertain whether the informant meant the ruins or the modern village of Palenque.

LINTON SATTERTHWAITE, JR.
University Museum
University of Pennsylvania
May, 1944

MAYA EPIGRAPHY: METHODS OF INTERPRETATION

There are different methods of interpreting Maya time counts. The Tun-Ahau method means that the Ahau element of an inscription refers to the *tun*; the Katun-Ahau method means that the Ahau element of an inscription refers to the *katun*.

The Initial Series inscription at Chichen Itza has been interpreted by a Tun Ahau method.

10.2. 9. 1. 9 9 Muluc 7 Zac
16.11
10.2.10. 0. 0 2 Ahau 13 Chen

According to this interpretation, 9 Muluc 7 Zac falls in a *current* 10 Tun, which Tun ends on 2 Ahau. Consistent with this Tun-Ahau method, the inscription on the Four Lintels at Chichen Itza, 9 Lamat 11 Yax, 13 Tun 1 Ahau, means that 9 Lamat 11 Yax falls in a *current* 13 Tun, which Tun ends on 1 Ahau. The date 9 Lamat 11 Yax, by this method can occupy only one long count position within a range of 18707 years.

4.6.12. 5. 8 9 Lamat 11 Yax
12.12
4.6.13. 0. 0 1 Ahau 18 Zotz

9 Lamat 11 Yax falls in a current 13 Tun, which Tun ends on 1 Ahau, but the long count, 4.6.12.5.8, is 2,283 years before the 10.2.9.1.9 of the Initial Series inscription. To meet this difficulty, regardless of consistency, a different Tun-Ahau method is applied to the Four Lintels inscription, thus:

10.2. 12. 1. 8 9 Lamat 11 Yax
1. 0. 16.12
10. 3.13. 0. 0 1 Ahau 18 Zip

10.15.15.12. 8 9 Lamat 11 Yax
17. 5.12
10.16.13. 0. 0 1 Ahau 18 Zac

11. 8.19. 5. 8 9 Lamat 11 Yax
13.12.12
11. 9.13. 0. 0 1 Ahau 13 Pop

The interpretation by the second Tun-Ahau method is highly hypothetical, with three possible Ahau dates, centuries apart, and irrespective of preference no one knows which date is meant.

The inscription on the Initial Series lintel at Chichen Itza correctly reads, 10.2.9.1.9, 9 Muluc 7 Zac, 10 Tun 1 Ahau. The Ahau coefficient definitely is One as has been determined since the earlier and erroneous surmise of a coefficient Two. This inscription has been interpreted by the second Tun-Ahau method.

The real relation between the Initial Series date and the next 10 Tun 1 Ahau dates is:

9.16.10. 0. 0	1 Ahau 3 Zip
5.19. 1. 9	
10. 2. 9. 1. 9	9 Muluc 7 Zac
7. 0.16.11	
10. 9.10. 0. 0	1 Ahau 3 Zac

These conditions are much less satisfactory; in fact, they make interpretation highly hypothetical. We do not know which of these two possibilities (9.16.10.0.0 or 10.9.10.0.0) is meant in the text, although for general reasons the latter date is preferable (*Studies*, p. 141).

This interpretation of the Initial Series inscription is the best that can be given by the Tun-Ahau method. It parallels the similar Tun-Ahau interpretation of the Four Lintels inscription and is equally erroneous because the Tun-Ahau method is fallacious.

The Katun-Ahau method is applied to the decisive Initial Series inscription, 10.2.9.1.9, 9 Muluc 7 Zac, 10 Tun 1 Ahau.

10. 2. 9. 1. 9	9 Muluc 7 Zac
16.11	
10.2.10. 0. 0	10 Tun
10. 0. 0	
10.3. 0. 0. 0	1 Ahau 3 Yaxkin, Katun ending

The long count position of 10 Tun is 10.2.10.0.0, and the Katun is 1 Ahau. 9 Muluc 7 Zac falls in a current 10 Tun of a current Katun 1 Ahau, which accords with 10 Tun, (Katun) 1 Ahau,—a fact that is conclusive.

The Katun-Ahau method is applied consistently to the Four Lintels inscription, 9 Lamat 11 Yax, 13 Tun 1 Ahau.

10.2.12. 1. 8	9 Lamat 11 Yex
16.12	
10.2.13. 0. 0	13 Tun
7. 0. 0	
10.3. 0. 0. 0	1 Ahau 3 Yaxkin, Katun ending

9 Lamat 11 Yax falls in a current 13 Tun of a current Katun 1 Ahau, which accords with 13 Tun, (Katun) 1 Ahau.

Katun-Ahau interpretation is not highly hypothetical but, on the contrary, is singularly definitive. A calendar round date with a positional determinant is limited to a single long count position for a period of more than seven thousand years. The Katun-Ahau method is simple, direct and coherent; it is sensible.

The new method of interpreting native time counts, first proposed by J. Eric Thompson, is fundamental to the proper solution of the Maya correlation problem.

R. B. WEITZEL
Washington, D. C.

NOTES ON CERAMIC TYPES IN SOUTHERN NEVADA

While there has been a great deal of archaeological excavation in the area now included in the Boulder Dam National Recreational Area in southern Nevada and northwestern Arizona, comparatively little of this material has been written and published.

Within this area, which encompasses about 2,700 square miles of desert and mountain land, there has been extensive occupation by various peoples throughout most of the past two thousand and more years. The National Park Service has conducted a survey and excavation program in the region in order to determine its complex archaeological history.

As it is perhaps the best means of readily identifying and separating cultures into their different phases, considerable attention has been given to the pottery. The study has brought out quite clearly the importance of tempering materials as aids in the recognition of differences in pottery wares, particularly in the separation of indigenous from imported wares.

Pottery wares in this peripheral section of the southwestern archaeological area consist chiefly of two types, Pueblo gray ware and Patayan brown and gray wares, the latter of which come in from the south. They are readily distinguishable, but more difficulty was encountered in breaking down the Pueblo gray ware into series and types. Study revealed that the gray ware was identical to a Washington Gray Ware which Dr. H. S. Colton of the Museum of Northern Arizona had previously found to center in the upper Virgin River area of southwestern Utah and to be closely related to Tusayan Gray and white wares which occur in northeastern Arizona and southeastern Utah. This was logical, as the natural path of migration for both ideas and peoples would have been from the main Pueblo area, around the northern end of the Grand Canyon, up the small tributaries of the Colorado River to the Virgin River, and down into southern Nevada along the Virgin and Muddy rivers. However, there arose the question as to whether or not there were identifiable local pottery types, deriving out of, but differing from, the closely related intrusive types from the north.

Further breakdown resulted in the discovery that many sherds otherwise typical of southwestern Utah varieties showed a major difference in tempering material. Fine to medium quartz sand was used as temper for the northern intrusive types, while in the local types, in addition to a certain amount of quartz sand, there also occurred relatively large amounts of a yellowish-green mineral—olivine—and, in many cases, garnet as well.[1] The presence of even slight amounts of green olivine and reddish-brown garnet crystals made identification of indigenous pottery types a simple matter.

Megascopic analysis of the tempering materials occurring in this local Nevada ware gave several interesting and unlooked-for results. The quartz grains were uniformly spherical and rounded, with dull and pitted surfaces, suggesting wind-blown material. On the other hand, the olivine crystals had rounded, polished surfaces and, in addition, were cut by fresh, sharply angular surfaces, indicating that the grains were originally rounded by water and were later crushed. This would seem to point to the conclusion that larger olivine crystals were crushed and ground in a stone mortar, later to be added to the quartz sand and used as tempering material. It is interesting and rather unusual to find that one of the ingredients used as temper had been deliberately sought for, ground up, and added to the naturally occurring quartz sand.

This fact definitely sets off the indigenous pottery types of the Boulder Dam National Recreational Area in southern Nevada and northwestern Arizona from the closely related pottery types occurring to the north and northeast throughout southern Utah and northeastern Arizona.

GORDON C. BALDWIN
Boulder City, Nevada
August, 1944

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[1] Dr. Harold S. Colton of the Museum of Northern Arizona has freely given a great deal of assistance in the identification of pottery types, while Mr. J. Schlocker, Junior Petrographer, U.S. Bureau of Mines, Boulder City, Nevada, has aided in the identification of tempering materials.

BOOK REVIEWS

Cross Sections of New World Prehistory. WM. DUNCAN STRONG. (Smithsonian Miscellaneous Collections, Vol. 104, No. 2. Washington: The Smithsonian Institution, 1943. 46 pp., 33 pls.)

The Institute of Andean Research was founded by a group of distinguished scholars in this country on the inspiration of Dr. Julio C. Tello of the Universidad Mayor de San Marcos, Lima, Peru, and as a result of his extensive tour of universities and anthropological institutions in the United States during 1936. Its purpose is to promote and coordinate anthropological investigations in archaeology, ethnology, physical anthropology, linguistics, and community studies throughout the Andean area and related regions. Certain projects previously started by institutions, museums, and universities interested in this field were incorporated. The funds, made available through the Art Committee of the Coordinator of Inter-American Affairs, were handled by the American Museum of Natural History. A number of publications and reports have resulted from the work done between 1937 and 1940; this paper, *Cross Sections of New World Prehistory*, by William Duncan Strong is the report of the Institute for the years 1941 to 1942.

The program for this period envisaged eleven field expeditions, ten of which were to be completed within the year. Each project was directed by a member of the Institute, serving without pay, who generally spent the summer in the field, selecting sites and arranging the details; the actual work was carried through by a supervisor, a qualified younger archaeologist. Cooperation between the students of archaeology from the United States and those of Latin America produced a profitable exchange, bringing the highly specialized techniques of our country into accord with local knowledge and methods. For the locations of these projects see Figure 31.

To Peru were allocated four expeditions. This center of major importance in the study of pre-Columbian civilization has never been investigated to the extent it deserves, either by Peruvian, European, or United States scholars, in spite of the tremendous wealth of undocumented material lodged in museums and collections throughout the Old and New World. Another project covered the northern coast of Chile, generally neglected by archaeologists, though significant because of the similarities and differences of its cultures when compared with those of its neighbors. Two other expeditions were assigned to the highlands of Colombia and Ecuador, where there are a number of cultures characterized by the presence or absence of traits that may have traveled from the great centers in the Andes and in Middle America. Venezuela and Cuba embraced one project for the study of diffusion and migrations. Another included El Salvador, that fascinating region where traces of southern Maya mingle with those from the Isthmus. Although Guatemala and Mexico have been much more intensively worked than the preceding locales, two investigations were carried out in Mexico in districts hitherto meagerly explored.

In general, less known regions were selected, and efforts were concentrated on those sites where immediate stratigraphic or historical results could be expected instead of on large and spectacular ruins where the time limit would have precluded such definitive assessments.

Project 1. Eastern Mexico.—Under the nominal direction of Dr. George C. Vaillant—as Chairman of the Executive Committee of the Institute, he also undertook a preliminary flying trip to discuss the plans in the various countries concerned and to negotiate working permits—Dr. Gordon F. Eckholm worked at the Huastec sites of Tampico and Panuco. In the latter, six cultural periods could be defined, and the Tampico-Panuco

sequence can now be correlated with the known sequences of Middle America. The first two Panuco periods appear to be related to the early Lowland Maya and the early Zapotec, while the material unearthed near Tampico throws new light on the relation

FIG. 31.—Map of Middle and South America showing general locations of Institute of Andean Research investigations in 1941–1942. (*Courtesy of the Smithsonian Institution and Dr. Strong.*)

between the pottery of the Huastec area and the early ceramics of the lower Mississippi Valley.[1]

[1] See also Gordon F. Eckholm, "The Third Round Table Conference," AMERICAN ANTIQUITY, April, 1944, pp. 443–444.

Project 2. Western Mexico.—Dr. Isabel Kelly carried out a program of survey and excavation in Michoacán and Jalisco, under the direction of Prof. A. L. Kroeber. At the request of the Instituto Nacional de Antropología é Historia, the first two months were devoted to western Michoacán. At least three distinct cultural "horizons" were found to be represented in the large collection assembled there.

In the state of Jalisco the operations were concentrated along the middle course of the Rio Armería. Through this excavation, ceramic complexes that had been tentatively established earlier were verified. The pottery of the region reveals intimate ties with Colima. By means of the newly gained knowledge of local ceramic complexes, it has become possible to correlate various archaeological horizons of western Mexico, which were the work of other projects. Now, however, this chronology appears to be too late to have played a significant formative role in the development of the cultures of our Southwest, and, unless an earlier horizon is discovered in western Mexico, it will be necessary to look elsewhere for contact corridors.

Project 10. El Salvador.—Dr. A. V. Kidder directed the work at Los Llanitos, Department of San Miguel, which was carried out by Dr. John M. Longyear III. A survey of material from private collections was added to that unearthed here. A great amount of pottery fragments and two caches containing a number of unbroken vessels were found, with indications that Los Llanitos was a one-period site, dating somewhere between the tenth and eleventh centuries. Although there was a ball court (the most southerly so far discovered) among the ten mounds investigated, influences from the Maya or from the cultures to the south and east were almost totally lacking.

On the other hand, in preliminary excavations carried out in the departments of La Libertad and Santa Ana, Stanley H. Boggs, assistant in this program, found several phases of building activity at Tazumal from which important discoveries relating to the frontier region of the Maya are expected. In addition to these results and indications, the work at Campana San Andrés in the Department of Santa Ana, which was begun by the Middle American Research Institute of Tulane University and taken over by Carnegie Institution of Washington, should be mentioned here. On a visit to the site in the early stages of the field work, this writer was impressed by artistic trends which were suggestive of the north, side by side with forms and patterns which showed more local invention.[1] With more extensive work, important revelations can be expected from El Salvador.

Project 5. Venezuela and the West Indies.—Dr. Cornelius Osgood, as director, and George Howard, as supervisor, studied local collections and material previously excavated and made numerous test trenches in Venezuelan sites. Intensive excavations were carried out in the Ronquín region on the Orinoco River. Six major ceramic areas were established, of which the Lake Valencia district is most typically Venezuelan. The western highlands show connections with the west, while tests along the lower Orinoco provide the first definite evidence of relationship in pottery styles between the mainland of South America and the West Indies. The material from the middle Orinoco and that from the Lake Valencia region at Tocorón both suggest two periods, the earlier at each bearing resemblances to that of the other site and to Trinidad, while the later period in each case seems more typically local in character.

In Cuba, Prof. Carlos Garcia Robiou participated as supervisor, with Dr. Irving Rouse as assistant director. Excavations were made at both the western and eastern ends of the island. The former, at Cayo Redondo, revealed information about the Ciboney Indians, a pre-ceramic, pre-agricultural group who may have had their origin in Florida. The work in the east, in the Maniabón Hills, was concerned not only with the

[1] See *American Anthropologist*, Vol. 42 (1940), pp. 712–713.

Ciboney but also with the Arawak, a pottery-making, agricultural group. The theory is that the Arawak, migrating from South America, drove the Ciboney into the remote corners of the island. An earlier Sub-Taino immigration from Haiti, previously unknown, and a later one, entering Cuba only a few years before the Conquest, were also traced.

Project 6. Colombia.—This work was directed by Dr. Wendell C. Bennett, accompanied by James A. Ford as supervisor. After consultation with Colombian savants, the Cali region of the Cauca Valley was selected for investigation, and, supplementing this, a general survey was made of important collections and sites in this country—among them, San Agustín. A short visit was paid to the deep subterranean tombs, discovered by Dr. Hernández de Alba, at Inzá, in the Tierradentro region, where circular stairways lead down into the interiors and the walls are decorated with carved relief and painted geometric designs. The more than four thousand photographs that were taken of local collections are deposited with the National Museum in Bogotá.

Although the excavations at Cali disclosed no evidence of large concentrated villages and none even of surface ruins, many tombs were discovered. The common type was hemispherical in shape and was entered from one side through a square shaft. Two major periods could be distinguished: the later, characterized by shafts about six feet in depth and by more elaborate pottery; the earlier, by tombs as deep down as twenty feet and by cruder ceramic work. Most of the vessels had apparently been made especially for burial.

Project 9B. Ecuador.—Dr. Fay-Cooper Cole was the director in this case, while the work was carried out by his assistant director, Dr. Donald Collier, and the supervisor, John V. Murra. After a survey which extended from Riobamba, in central Ecuador, southward to Loja—a distance of about 180 miles with a range in altitude from 2,500 to 14,000 feet—the Cañar Valley was chosen for excavation. The stratigraphic sequence revealed that the Cañari Indians settled the valley between 1000 and 1200 A.D. They had very fine pottery but at first made little use of metal; later, however, they fashioned copper axes and elaborate gold ornaments. About 1400, influence from the mountain people to the north is apparent, and fifty years later they came under the sway of the Incas. The Cañari culture seems to have had Andean roots; no evidences were found of an infused Mayoid culture, as was suggested some decades ago by earlier investigators.

Edwin N. Ferdon, Jr., for the School of American Research, has also made a valuable contribution to the archaeology of Ecuador, and the efforts of local savants are constantly calling attention to the importance of further work in this key region.

Project 9A. Northern Highlands of Peru.—Dr. Theodore D. McCown supervised excavation of the ruins of Marca Huamachuco and Viracochapampa, together with other sites. At the former ruin, two distinct styles of architecture were encountered. The earlier period is characterized by narrow structures rising as high as three stories, and associated with it are distinctive stone sculptures, a plain red-slipped ware which is occasionally ornamented with negative-painted designs in black, and a finer white-paste pottery with "cursive-painted" decoration. Though the later period had the same general type of building, the stones were smaller and the walls thinner. The ceramics include a plain ware decorated with appliqué, punched and incised designs, and a ware crudely painted in red and black. In spite of a suggestion of Inca influence in the pottery, this is believed to antedate the true Inca period.

Viracochapampa seems to represent the latest period in this region. The rectangular plan of its architecture is suggestive of the Inca, but local style persists in the long narrow galleries. The pottery is very crude and differs from the early period of this region. No Inca polychrome ware or imitations of it were found.

The stone carvings encountered were found to be distinct in style from those at Chavín de Huántar. In the pottery, middle and late Chimu influences are discernible,

and there is even a certain relationship to Ecuador in the negative painting, but no Tiahuanaco or Epigonal styles were encountered, though these are reported from sites farther to the north. The absence of true Inca styles is puzzling, but no judgment can be reached until more work has been done.

Project 7. Southern Highlands of Peru.—Here Dr. Alfred Kidder II directed the work, with John Howland Rowe as supervisor, Marion Hutchinson Tschopik as assistant, and Dr. Luis E. Valcárcel aiding materially. Ten sites of the pre-Inca period on the Peruvian side of Lake Titicaca were examined, and it became clear that the Classic period of Tiahuanaco culture did not occupy the entire lake basin and vicinity. The northern sector of the area seems rather to have been the home of the Pucara culture, closely related to Tiahuanaco but not an extension of it, the two appearing rather to emanate from a common Titicaca culture. Mrs. Tschopik investigated numerous sites and excavated on a smaller scale to determine local sequences and associations. According to her findings, most of the "Chullpa" sites are of Inca date.

Mr. Rowe worked in and around Cuzco, finding traits which connect with Chavín, Pucara, Tiahuanaco, and Chiripa. Cuzco Inca ceramics were classified and a detailed study of the Temple of the Sun was made.

Project 8. Central Coast of Peru.—Drs. Samuel K. Lothrop and Julio C. Tello acted as co-directors of the project. Dr. Marshall T. Newman was supervisor. A study was made of Paracas textiles and pottery from the Necropolis and Cavernas cultures. The textiles of the former, found in subsurface structures, have complex designs somewhat similar to polychrome Nazca pottery; their pottery, on the other hand, is rather simple and lacks polychrome decoration. The material of the Cavernas culture was discovered in deeper tombs. Its textiles are less elaborate, but its pottery is subtly incised and painted. Here a feline deity closely related to the style of Chavín de Huántar is to be found. The monograph on the textile findings, to be published under the auspices of San Marcos University, will be of great interest even outside the scholarly field.

In the second aspect of this project, crania and other skeletal parts from the above site were studied, together with skeletal material encountered in excavations at Pachacamac. The series of freshly excavated skulls now covers the entire span of known coastal Peruvian cultures. Because of the analogy to Middle America, it is of special interest to mention that cranial deformation began in the earliest period but was abandoned in the latest. With few exceptions, one basic physical type appears.

Project 3. Central Coast of Peru.—Here Dr. William Duncan Strong was the director, Dr. Gordon Willey, supervisor, and John M. Corbett, assistant. A stratigraphic cut was made in a large refuse heap near the Temple of the Sun at Pachacamac. Two meters of abundant Inca refuse, including associated late local styles, were found, and below this were eight meters of deposit containing a ceramic style designated as Pachacamac Interlocking, a hitherto unreported type called Pachacamac Negative, and still earlier traces of Chancay White-on-red. A sequence not previously found at Pachacamac was thus revealed.

North of Lima, in the Chancay Valley, a series of similar stratigraphic cuts were made. The discoveries here not only add earlier coastal types of ceramics to the Pachacamac-Chancay sequence but also, on the basis of clear superimposition, reverse the conclusions of earlier excavators as to the order of appearance.

Excavations were carried out also in the ancient shell heaps of the valleys of Supe and Ancon, establishing beyond much doubt the great antiquity of their early ceramic types, as well as a relationship to Chavín in the central highlands, to the Cupisnique style to the north, and to Cavernas on the south coast. Graves from this period and structures that may represent the earliest temples in coastal Peru were investigated.

Project 4. Northern Coast of Chile.—The project was under the nominal direction of Dr. William Duncan Strong, with Junius Bird as field supervisor. Intensive excavations were carried out between Arica and Coquimbo. The indications are that the coastal desert was first settled by a fishing population with simple artifacts that are surprisingly uniform in widely scattered localities. A sharp break appears in the cultural pattern with the arrival of another non-agricultural fishing population employing different equipment. Agriculture was apparently introduced from coastal Peru. Subsequent to this phase, certain changes in pottery styles are evident. Tiahuanaco traits are rarely found on the coast here; they seemingly are confined to the highland regions. It is remarkable that Tiahuanaco influence does not occur in Chile until relatively very late.

Excavations in the Coquimbo-Serena region indicate that the well-known Diaguita culture arrived there fully developed, presumably from what is today Argentina. With the additional knowledge obtained from these excavations, the long-discussed problem of a paleolithic-like culture in Chile is found to lack stratigraphic evidence.

This digest of the various projects, necessarily very condensed, gives only an approximate idea of the vast areas investigated and the time spans covered, but, even so, it will serve to draw attention to the advance in our knowledge achieved through the work. Details of the individual projects and a complete list of publications concerning them can be found in Dr. Strong's report. A goodly number of the monographs have already appeared, and the rest are to follow in due time. But, to quote Dr. Strong, "the full significance of such works will be realized more gradually as the conclusions therein contained find their way into more general treatises dealing with the universal or unique factors implicit in the rise and fall of New World civilization and as these in turn are evaluated against the total background of culture history. This is the ultimate aim of all archaeology that is more than a pseudo-scientific cloak for antiquarianism."

It is the assumption of most anthropologists that a prehorticultural mode of life underlies the agricultural civilization of the pre-Columbian peoples, just as is demonstrably the case in the Old World. Recent excavations in the southeastern and southwestern United States, even though not yet satisfactorily synchronized, tend to substantiate this, and the present program of the Institute of Andean Research has produced evidence of such a major sequence also in northern Chile. It would appear that these pre-agricultural manifestations in the peripheral regions overlap in time the great agricultural developments lying between them.

One striking difference, however, from the rise of Old World civilization is becoming more and more apparent in the study of the New World. This is a seemingly very early but developed cultural horizon, stratigraphically demonstrated on the Peruvian coast, in Spanish Honduras, Guatemala, certain sections of Mexico, and the southern United States. "It is too soon to claim that the lowest horizons between Central Peru and the southeastern United States are related in time and culture. However, there are striking resemblances between several of them, and it is significant that the various archaeologists who have recently worked these horizons out . . . all agree as to their relative ages."

As stated before, the program of the Institute does not embrace the heart of Middle America, where, proportionately, much more work has already been done; the very title of the organization points to other areas. However, the projects allotted to some practically unworked border regions thereabout have borne positive fruits. Along the northern border of Mexico, for instance, where the characteristic traits of that land peter out and "fingers" of our southwestern-southeastern cultures can be observed, considerable new information was collected. Around El Salvador, the southern limit of Maya influence, with the extended strip of Pipil culture, additional important clues were found. Further

work in these regions is urgent, as is also the case in Costa Rica, Panama, and Colombia. In certain districts, stylistic intermingling is so pronounced that undocumented objects in collections can be assigned only very tentatively. The confusion is augmented by the fact that, although the Republic of Panama was separated from Colombia as an independent entity as long ago as 1912, numerous objects that came into the possession of museums and private collections before that date are still ascribed to Colombia. As late as the mid-1930's this writer saw golden objects in the British Museum labeled "Colombia" which are now recognizable as from the Coclé culture in Panama.

In his "Preview of Results," Dr. Strong brings up certain similarities in the artistic vocabulary of various regions, such as the appearance of the jaguar or puma god in more than eight cultures, where it differs only slightly in its representation. In such observations is apparent the realization of the importance of giving full attention to artistic trends. In the material now made available, many other resemblances can be noticed—to mention only one, the compositional and figural similarities between the Nariño style of Colombia, recently published, and the open bowls of Coclé, Panama.

This reviewer is glad to see the term "culture historian" used by Dr. Strong in connection with the study of pre-Columbian civilization. With the growing knowledge in this field and the amassment of material that can be documented, it is more and more evident that two types of archaeologists are desirable for the work. The one excels in field work, is expert in the *finesses* of excavation and preservation of material, and knows how to live under the generally unfavorable conditions in areas where much of the field work must be done. The second group comprises those who, through the accumulated knowledge of year-long contemplation and comparison of material from widely separated cultures, have gained a panoramic view and can build the many small units into a larger picture, the expanding and imposing mosaic of pre-Columbian civilization. This problem has been notably well faced in the organization and personnel of the Institute of Andean Research.

The activity of European archaeologists has been crippled for years by the war and its shadow, cast far ahead. However, our own institutions, cooperating with one another and their Latin-American colleagues, have carried on and forward. In several cases, projects already started by universities or museums in this country have been integrated into the program of the Institute of Andean Research. Understandably, a certain absence of uniformity is noticeable, not only in the exterior of the published volumes—such as format, typesetting, illustrations, etc.—but also in approach. While this may have drawbacks, the comparison of the nuances from the point of view of technique or presentation can also prove advantageous.

This is the first time in the history of pre-Columbian archaeology that such vast areas have been worked, and, although under the supervision of differently thinking brains, in the coordinated framework of one central committee a synthesis of unprecedented scale has been achieved. The echo in Latin America of the Institute's work is important in many ways. Not only were most projects in constant touch with archaeological circles in the respective countries, but frequently solid cooperation was achieved. Indeed, the contribution of Latin American archaeologists and scholars to this program is inestimable and should receive special mention here. Friendships commenced in field and museum will flourish also in after-years, and the exchange of experience and knowledge will be fruitful to both sides.

When all twenty-two reports have been published—in them forty-five scientists and students have participated—the bibliography of pre-Columbian archaeology will have received a most respectable addition—respectable in any part of the New and Old World. With these volumes, not only will a great step have been made toward the creation of

an over-all panoramic view of our pre-Conquest cultures, but paths will have been laid down on which others can go forward to more distant goals. It is more than satisfactory—it is stimulating and sometimes even enthralling—to look through these books, with their riches of new information and pictures of unknown sites and objects. Whoever has any interest in the pre-Columbian past of our hemisphere must be grateful to all who contributed so positively toward enlarging our knowledge of it.

PÁL KELEMEN
Norfolk, Connecticut
July, 1944

The Fort Ancient Aspect, Its Cultural and Chronological Position in Mississippi Valley Archaeology. JAMES BENNETT GRIFFIN. (University of Michigan Press, 1943. 376 pp., 157 pls., 18 figs., 10 maps, 14 tables.)

The Fort Ancient Aspect is probably the most comprehensive treatment of a single major North American archaeological cultural manifestation ever attempted. As a grand scale effort it may be expected to have reached its objective at some points and fallen short at others. This reviewer hastens to say that in his opinion the defects comprise but a small fraction of the whole, and that this work constitutes a pinnacle well above "the common works of man."

"This study," as Dr. Griffin states (p. 2), "is the result of library and laboratory research," conducted over a number of years, utilizing the archaeological materials preserved in scattered museums and collections in the eastern United States, derived from thirty-three sites of the Fort Ancient Aspect, a late prehistoric and early protohistoric manifestation, localized in the Ohio drainage basin (southern Ohio, northern Kentucky, and southeastern Indiana), with probable extensions into the lower Kanawha Valley in West Virginia.

Griffin's larger analysis supports his preliminarily published conclusions on this subject and sustains his postulated subdivision of the aspect into four foci—Baum, Anderson, Feurt, and Madisonville—which are differentiated, to a large extent, on the basis of ceramics. With respect to its broader affiliations, the author states (p. 208), "It is felt that the evidence from this study is sufficient to support the already accepted hypothesis that the relation of the Fort Ancient Aspect to the Oneota Aspect is such that together they constitute an Upper Mississippi Phase."

"The main body of the report contains a series of brief descriptions of the artifacts recovered during the excavations and the features of each of the individual sites or of the surface collections from them. A comparative trait list in which the archaeological traits for most of the sites discussed are presented in one table has been compiled. In the discussion of the artifacts from each site, arbitrary divisions based upon the type of material used in their construction have been made, in order to simplify treatment and comparison and to eliminate the use of subjective groupings based upon such terms as 'art' and 'ceremonial.' The greatest possible correlation with the specific type of object recovered at one or more sites is desirable in a comparative trait list. The traits listed in this report only approach this desired standard and frequently comprise classes of material" (p. 3).

In his chapter on "The Association of Historic Tribes with the Fort Ancient Area," Griffin carefully reviews the documentary sources and critically evaluates the several opinions concerning ethnic identifications, concluding with the statement that, "A careful examination of the early sources has failed to substantiate as historically valid any claim that the Fort Ancient area was occupied by either Iroquoian, including Cherokee, or

Siouan tribes. It is theoretically possible that members of these two linguistic stocks may have occupied this territory at some prehistoric time, but as yet there is no proof " (p. 34).

Other chapters include "The Chronological Position of the Fort Ancient and the Hopewell-Adena Occupation," in which the errors of temporal orientation of earlier workers are pointed out and the indisputable fact of the priority of the Adena-Hopewell is established; and "The Cultural Interrelations Between the Fort Ancient and Iroquoian Aspects," in which Griffin shows that, while there are "certain fundamental and cultural similarities" between the two, "it is improbable that the former was ancestral to the latter" (p. 239), with which conclusion the reviewer is in complete accord.

Four chapters (XII–XV) are devoted to a discussion of the relations of the Fort Ancient culture to the southeastern United States, the Middle Mississippi phase, central Indiana, and the Upper Mississippi phase, respectively. Here Griffin, working with the limited data available, traces the probable temporal, spatial, and cultural relationships of the Fort Ancient aspect, and succeeds in conveying some understanding of the composite nature of this manifestation. The newer conception of the non-static nature of culture, developing by accretion of traits from sundry sources on varying time levels, accompanied by recombinations and interpenetrations of the discrete elements, is emphasized here and elsewhere, in contrast with the older notion of more or less crystallized and circumscribed aggregates, in motion from place to place.

The basic nature of the admittedly somewhat arbitrarily derived Fort Ancient aspect is summarized in the writer's short and cautious chapter devoted to "Speculations," where he states, "The Fort Ancient Aspect then represents a Middle Mississippi offshoot which merged culturally with a basic Woodland group already tinged with Mississippian traits. This process was interrupted by the pressure of the Iroquois from the northeast, and the southeastern Fort Ancient sites were modified as a result of the pressure of the Europeans on the Indian tribes of the Piedmont and mountain area of West Virginia and Virginia. The Madisonville Focus lasted until the period between 1670 and 1690, when its cultural unity was destroyed by the Iroquois and by the attraction of the Indians in the area to trading centers such as those of the Illinois Valley, the Middle Atlantic area, and the Southeast. The evidence does not suggest that Fort Ancient people were responsible for Iroquoian, Fisher, or Oneota Aspect sites" (p. 308).

Four appendices complete the work. "A" consists of an excellent discussion of the uses and abuses of the McKern taxonomic system; "B" and "C" contain detailed descriptions of types of pottery; while "D" relates to several subjects—ceramic type distribution; fauna and flora associated with the Fort Ancient aspect; and trait tables showing the distribution of Fort Ancient non-ceramic traits by foci, aspect, and components.

In this reviewer's judgment, the one major flaw in this definitive work on the Fort Ancient aspect is the marked disparity in mode and intensity of treatment accorded the pottery and the non-ceramic traits. Considerations of ceramics are, of course, handled with consummate skill and complete adequacy. The pottery of the aspect is superbly described in the text, numerous descriptive tables, and illustrations, in the latter to the point of prodigality. It is otherwise with the non-ceramic constituents. In places the author seems definitely to hurry through the "running description," as he calls it, of non-pottery traits, including burials, cache pits, house floors, etc., to get on more congenial ground. Sufficient illustrations of all characteristic traits of the aspect would be very desirable in such a source book, but the number of illustrations of non-ceramic artifacts is meager, the great majority of the types mentioned in the text appearing nowhere.

Portrayal of non-ceramic traits is particularly necessary inasmuch as no standard

nomenclature is yet in use. Griffin himself points out (p. 201), "Thus, different terms for the same specimen, or the same term for different specimens, can profoundly modify the classification of archaeological sites, especially when the determinants are selected." What, for instance, is the "hair-spreader," often referred to as a characteristic of the Fort Ancient aspect, but nowhere, so far as I can tell, either described or illustrated?

Moreover, the captions to those traits illustrated are sometimes inadequate—e.g., Pls. XV; XXVII, particularly Figs. 7–10; XXVIII; LI; LII; LXXIX. The use of a scale, too, would have been helpful in those cases where size is not indicated, especially where not all of the objects are on the same scale—e.g., Pls. XV, XXXI.

On page 3, Griffin has given his explanation for this differential treatment in the following statement: "Pottery has received the most detailed consideration in this study and is the subject of most of the illustrations, because in both the New World and the Old World it has been demonstrated that pottery is the most important single factor in the interpretation of archaeological cultural relationships." While no question will be raised here of the fundamental truth of this statement, the reviewer strongly believes that in the definition of a "cultural conclave" (to borrow a phrase from Griffin), non-ceramic traits must be thoroughly considered as well, lest we have as the end product of our research what amounts essentially to a series of elaborately named and weakly supported ceramic sequences. There is already ample evidence at hand from most sections of the country to show that projectile points and many other classes of stone and bone implements, including even the humble hammerstone, have varying degrees of deterministic value in culture differentiation.

It is unfortunate that frequencies for traits other than pottery could not be given in this work, since frequency has much importance in determining the relative significance of a constituent element. To illustrate from the book at hand, the grooved stone axe is listed from four components, the plummet from five, and the bell pestle from seven, but Griffin fails (inadvertently?) even to list the first in his trait table (Table VII) and repeatedly expresses doubt of the intrinsic place of the last-named trait in the culture, although it is present in all foci (see pp. 72, 123, 197, 231). The "hair-spreader," on the other hand, occurs in but one or possibly two components, pertaining to one or possibly two foci, yet it is listed as a trait of the aspect (p. 368). Is this because the much greater frequency of the latter trait removes this doubt? Such matters cannot be determined from either Griffin's text or tables.

In his chapter, "Comments on the Classification," the author states (p. 335), "The concepts 'determinant,' 'determinant trait,' 'determinant complex,' 'diagnostic,' 'diagnostic trait,' 'diagnostic complex,' and 'link traits' have not been seriously employed in this paper, partly because of the confusion and contradiction in the present use of such jargon and partly because there was no apparent need for such terms." This, however, does not actually seem to be the case. While eschewing the terminology, Griffin has employed the concept of deterministic or diagnostic trait and complex. Granted there may be "few cultural factors other than specific pottery types which are peculiar to the Fort Ancient Aspect" (p. 5), the particular combination and proportion of these traits constitutes a deterministic complex for the Fort Ancient culture.

It seems to this reviewer that these concepts have a real place in culture analysis. Such traits as "shell spoons," "disk-shaped beads," "bone beamers," "antler spear points," and "flint celts," to mention a few from the author's trait list (p. 369), surely have greater diagnostic value for the Fort Ancient aspect than "fireplaces in village sites," "cache and refuse pit," "artifacts with some burials," "pipes made of clay," "whole bone awls," "bone splinter awls," etc. (p. 369). On this basis the trait list for the aspect could probably be considerably shortened, in which form it would be more consistent with the McKern system.

Insofar as I can determine, the number of factual errors marring the work is surprisingly small in view of its magnitude. Dr. Griffin's keenly developed critical faculty, mastery of the scientific technique, and passion for precision are amply evident throughout. Omitting picayune criticisms and minor errata, there remain a few points which merit correction. For example, the true celt is abundantly present in the New York Archaic (p. 198); and the association of the basic triangular point with side-notched, corner-notched, and stemmed points occurs in the Laurentian, Middlesex, and Owasco aspects in the New York area, and is thus not unique for the Fort Ancient aspect (p. 198).

Two controversial points, raised by the author on page 307, remain briefly to be considered. The first is Griffin's statement that the temporal and cultural equivalent of "The Intrusive Mound Culture of Ohio, which is associated with the Point Peninsula Focus of the northwestern New York area, appears to be absent from the lower Ohio and northern Mississippi valleys, so that there is an even greater hiatus between the disappearance of Hopewell and the emergence of the Mississippi Pattern in the west than in the eastern Ohio-Mississippi area." Actually, the Arvilla focus, Red River aspect, of Minnesota shares numerous diagnostic elements with the Intrusive Mound and Point Peninsula cultures.

Griffin's next sentence, "Is the Intrusive Mound culture a cultural as well as chronological step to the Fort Ancient culture?" reveals his often heroic attempts (more evident in certain other articles) to demonstrate cultural continuities. While the reviewer realizes the necessity of this approach for the understanding of cultural etiology, he feels that its application should be kept within the legitimate limitations of the data; in its reactionary and extreme form it may become highly misleading.

Since the statements to which these two last criticisms apply are embodied in the "Speculations," they do not detract from the immense usefulness of his classic volume. When a sufficient number of such master studies shall have been made available, the synthesis of American prehistory on an objective basis will at length be possible.

WILLIAM A. RITCHIE
Rochester Museum of Arts and Sciences
July, 1944

The Pre-Iroquoian Occupations of New York State. WILLIAM A. RITCHIE. (Rochester Museum Memoir, No. 1. xvii+416 pp., 165 pls., 6 figs. Rochester Museum of Arts and Sciences, Rochester, New York, 1944.)

New York state has been among the most fortunate of the eastern states in having had able and industrious students of her prehistory for almost fifty years. The imposing volume being reviewed is the culmination of this research. The author is the present spokesman of the historical line of Beauchamp, Parker, and Ritchie. While Ritchie can be classed as belonging to the illustrious company of his predecessors, he is not genetically related to either of them. This volume is not only an able summary of the cultural grouping the author recognizes at present in New York, but is also a primary source for a number of important sites. The work of the Rochester Museum since 1925, in a general program inaugurated by Parker, has been largely concentrated in central New York. The eastern, southeastern, and southwestern areas have been somewhat less intensively studied. This major contribution to American archaeology analyzes, synthesizes, and interprets the archaeology of New York state which antedates the Iroquoian period: it furnishes an example which other eastern states would do well to follow.

The large number of plates, illustrating an unusually high percentage of the items from the sites, are a distinct aid in following Ritchie's discussions of the objects in the text and in utilizing the trait lists. Non-professional students will certainly benefit from

the plates and the descriptive terminology employed in plate captions and in text. I encountered some difficulty in using the plates because of lack of definition of the objects, a fault which is probably the result of the method of reproduction.

The field covered by Ritchie includes a wide variety of material and covers a considerable range in time. It is inevitable, then, that there will be differences of opinion among competent observers regarding the interpretations presented in this volume. It is to Ritchie's credit that he has included so much in his work and has had the courage to express his views. Students concerned primarily with the details of northeastern archaeology can get Ritchie's ideas from the volume and should compare them with statements made by various reviewers.

There is a résumé of the earlier archaeological work in the state which reviews the former cultural groupings and chronological arrangements, and indicates briefly their resemblance to the findings of the present volume. The brief section on the historic tribes of New York is devoted largely to quotations from early descriptions of Algonquian groups in southeastern New York. It is emphasized that almost nothing is known of the archaeological culture of these Indians at the period just prior to and during the early contact period. Since this is true, and since the Iroquois are not seriously emphasized, it is difficult to understand how the historical approach was used at all in the study. Iroquois archaeology is given in a brief summary which precedes the discussion of the Owasco aspect. There is an understandable but unfortunate tendency to confuse the problem of Iroquoian prehistory in the broad sense with its smaller segment, the archaeological history of the Five Nations. It is to be hoped that a research program can soon be formulated which will attack "the last scene of all"—the Iroquoian problem. If such a program is undertaken, it will benefit considerably from the excellent progress Ritchie has made in understanding the cultural forerunners of the Iroquois in the New York area.

The Owasco aspect is composed of a series of sites possessing a significant number of common characteristics. These sites can be further subdivided into a Canandaigua focus and a Castle Creek focus. The former is presented as a less complex, more widespread cultural entity which is ancestral to the Castle Creek focus. This division, Ritchie believes, developed in the upper Susquehanna and upper Delaware drainage areas, perhaps as the result of Iroquois influence. That Ritchie is not certain on this point is shown by his phraseology in the following instances. On page 29, "Iroquois connections are postulated upon typological considerations, for while pits and burials of both peoples have been opened on the same sites, *their contemporaneity has not been established*" (italics mine). On page 33, he speaks "of the postulated transition of Castle Creek from Canadaigua focus under Iroquois inspiration"; on page 41, he says "this assumed diffusion from remote Iroquois sources"; and on page 52, "an apparently progressive increase in the strength of Iroquois influence." On pages 56 and 57, there is a discussion of the problem of Iroquois-Owasco relations in which it is apparent that Ritchie leans to the interpretation of Castle Creek as ancestral Lenape or Delaware. At the same time, there is no attempt to minimize the number of resemblances between Owasco (particularly Castle Creek) and Iroquois. These cultural correspondences are brought out in the text dealing with Owasco, especially on pages 41, 46, 101 (physical type), and 327. I am impressed by these resemblances and by the relative absence of material in other areas which can be early Iroquoian. If Castle Creek is not the culture of the proto-Iroquois, or of some division of them, it certainly belongs to groups exhibiting strong cultural connections both with developed Iroquois and with sites to the west which might also be "early" or developmental Iroquois. This assumed Iroquois contact in Castle Creek times may have occurred in the Susquehanna Valley. Do we know, however, that occupation to the west by Canandaigua peoples had ceased? Late Canandaigua possesses concepts similar to those of Castle Creek, and the transfer might have been effected via a Castle Creek in-

fluence on Canandaigua or via unspecified influences on both Owasco foci, which also preceded any true "Iroquois" culture. "Iroquois" culture may be viewed as the culmination of a general trend in the northeastern area rather than the source of that trend.

Ritchie's comments on page 52 as to the possible origin of Owasco are good. They should be considerably developed in the future.

The singularity of the Delaware interest in representations of the human face has been overdone; the Iroquois have also evidenced considerable interest in such art forms.

A pipe with two modeled human faces from Upper Queen Esther's Flats just south of Athens, Pennsylvania, can hardly be ascribed to Castle Creek since the site belongs to the Susquehannock-Andaste division, among whom it was common to put a representation of a human face on the rim of a vessel. The Castle Creek pipes illustrated by Ritchie are simply further evidence of the strong cultural continuity from Castle Creek to Iroquois.

If the basketry from Castle Creek is really coiled, it is the first good evidence of prehistoric coiled basketry in the eastern United States of which I know.

The interpretation of the use of a palisade with exterior ditch as a less "diagnostic" element in common between Owasco and Iroquois might be questioned.

A good-sized section of the vessel illustrated on page 39 is in the Ceramic Repository. The impressions on the upper rim and those on the shoulder of the vessel and on the effigies are cord-wrapped string or dowel, and not dentate stamp. I suspect that Ritchie has overemphasized the strength of dentate stamping in his Owasco sites.

The Canandaigua focus covers quite a range of territory, from the Genessee to Albany, and includes some twenty components. Ritchie suggests that eight of these belong in an early Canandaigua period, while later sites within the same focal group, such as the Canandaigua site, reveal incipient Iroquois ideas. It is probable that this interpretation could be more fully developed, since the pottery from Jack's Reef (Canandaigua focus, p. 73) has quite a number of striking resemblances to that from Kipp Island (p. 131) called Point Peninsula focus, while some of the pottery from other early Canandaigua sites resembles the pottery from Lamoka. There is clear evidence in the pottery from the Canandaigua site that it not only has its cultural origins in Pt. Peninsula and Laurentian times, but also that it is almost Castle Creek. The site at Westfield, New York, worked by R. P. Wright, is similar to Canandaigua, with a probable later occupation which is closer to Iroquois, but which has strong connections with Monongahela Woodland. The obtuse-angle steatite pipes with flattened, broad stem and raised ridge along the stem hole which come from Jack's Reef and Pillar Point are probably physically derived from the middle Appalachian area.

In the report on the Canandaigua site, Ritchie mentions the presence of rectangular houses and points out the general use of the "Iroquois" long house in the northeast. This is not referred to in the summary of the Canandaigua component in this volume or in the comparison of Owasco and Iroquois.

As was observed in the report, the recognition of a "Coastal aspect" divided into "Early" and "Late" foci is a convenience based on preliminary examination. The use of the aspect concept apparently reflects certain geographical factors, while the foci seem to reflect temporal ones. No attempt is made in the report to justify the allocation of these two foci to a single aspect, and in all probability such sites as the ones producing the vessels illustrated on page 105 are as closely connected culturally to Vergennes as they are to such a site as Aquebogue on Long Island. In the "late" grouping some of the sites are local expressions of Castle Creek, while others appear to warrant inclusion in an "Iroquoian" archaeological division.

The Vine Valley aspect (?), as well as the New York focus of the Hopewellian phase, is conceived by Ritchie to have been "introduced from the Great Lakes area, perhaps

directly through Ohio." The difficulty of adequately understanding these units is due, as Ritchie recognizes, to the fact that they are "almost exclusively a mortuary complex."

Of the elements listed as diagnostic of Vine Valley, few if any are actually limited to that division. There is probably more Point Peninsula-like material outside of Ohio to the north and east than there is within the boundaries of that state. From this standpoint, the Intrusive Mound culture would be more likely to represent diffusion into Ohio during the general Hopewell period. It is unlikely that southern Ohio is the center of origin of either a migratory Point Peninsula group or the center for the diffusion of such ideas to the north. The presence of Hopewell items and concepts in some Point Peninsula sites, plus the early Point Pensinula sites without those concepts, suggests that this division was present in New York before, during, and after the Hopewell occupation in Ohio. This is implied in the time chart on page 10.

The difficulty in adequately defining the Point Peninsula focus on the basis of traits which are held in common and which also differentiate Point Peninsula from other units is disclosed by an examination of the number of sites out of a total of 15 in which the Point Peninsula "markers" are found: hafted beaver incisor cutting tool (11); platform pipe with straight (3) or curved base (2); birdstone with broad ridged base (2) or of narrow bar form (2); large antler comb (5); large flaking tool (8); thin side-notched point (12); extremely thin trianguloid cache blade (10); tubular pipe of clay (2) and stone (1); unworked or partially modified sections of bone or antler (13); decoration of pottery by means of roulette or rocker-stamp (2); and frequent use of liberal quantities of red ochre with burials (5). The cultural connections of this list are with middle western Hopewellian. Many of the items on the list of New York Point Peninsula traits not present in the Intrusive Mound culture of Ohio also have cultural affinities with middle western Hopewellian.

Another culture group in the Great Lakes area which has cultural ties with the Vine Valley aspect is the Glacial Kame culture. It is to be doubted that the Bowman mound near Linville, Virginia, is to be assigned to Point Peninsula, although the combs certainly indicate a connection. There are similar combs from New Jersey in the collection of Mr. Charles Philhower of Westfield.

Within Point Peninsula, Ritchie "suggests a dual or tripartite division" which reflects his interpretation of some sites as belonging to an early, a middle, and a late time period. This seems reasonable, and Ritchie will probably develop this thesis more adequately in the future. I believe that Point Peninsula pottery has also been misinterpreted as far as the proportion of dentate stamping is concerned. Pottery from Kipp Island in the Ceramic Repository has very little of this surface treatment. The few sherds with this decoration correspond nicely to Mandeville Stamped of Tchefuncte and to the simple dentate-stamped pottery assumed to be a relatively early style in the Illinois Valley. As Ritchie points out, the two types of pottery from Vinette, an early Point Peninsula site, are similar in many respects to the pottery from the Oberlander and Robinson sites. When it is also realized that Ritchie's Type 1 pottery is found on a Middlesex site (p. 192), there can be seen in the area a ceramic sequence which parallels that observed in the northern Mississippi Valley.

The cremated burial at Jack's Reef, which is assigned to Point Peninsula, exhibits strong Hopewellian connections in the platform pipes and shark's teeth.

For the presence of corn in Point Peninsula, Ritchie cites one cob from a burial in a pit at the West Rush component. The artifacts with the burial were two thin broken blades, while "the many objects picked from the surface of this field indicate occupations at several periods" (p. 126). If this be true, can the cob be definitely attributed to Point Peninsula? Ritchie's reference to Setzler's comments on the absence of corn in Hopewell

sites in Ohio could have been eliminated on the basis of my review of Setzler's article alone.

The late sites of Point Peninsula are particularly interesting in the number of traits they have which are also found in admittedly different proportion in the Canandaigua focus.

The presence of brachycephalic elements in Middlesex, Brewerton, and Frontenac is significant in the history of the physical type of the population in the area. This may be connected with the Centralid population of the Adena occupancy of the Ohio Valley and the Red Ochre people in Illinois. The absolutely amazing assurance with which some students attribute any round-headed population in the eastern United States to Middle American influence runs counter to the concept of increasing brachycephalization in the American Indian as representing an increase of the more definite Mongoloid strains. This Mongoloid strain is, in turn, presumably derived from Asia, but not across the central Pacific.

An interesting feature of the pottery from New York Hopewellian sites is the absence of the stamped body decoration which is associated with that "phase" throughout most of its distribution. Such stamped decoration apparently is earlier in New York than is the Hopewell influence. On the basis of the data presented, the assignment of the Liddydyke, Kipp Island, Bamber, and Kine mounds to Hopewellian might be questioned. My examination of the pottery from the Sugar Run site, discussed by Ritchie on pages 204 and 206, leads me to doubt the Hopewellian classification or assignment of the mound, irrespective of the temporal position of its builders. The late material at the site belongs in the Monongahela Woodland aspect.

The Orient focus is one of the most interesting of the more recently recognized northeastern cultural groups. I am not impressed with the suggested resemblances, "chiefly in the manner of occurrence," to what should have been called the "Rancocas focus" in New Jersey. Connections with some of the local "Early Coastal" manifestations in the immediate area, as Ritchie suggests, is a better bet for the future. The practice of ceremonial destruction of burial goods is related to other similar concepts in the eastern United States, from pre-pottery levels to Hopewellian.

Ritchie's Laurentian aspect (?) is the most controversial cultural group presented in the volume. The reviewer questions the correctness of the use of the classificatory concept "aspect" for this division. The foci, except for Brewerton, are weak, and the demonstration of the degree of unity of the widespread groups Ritchie places in a Laurentian aspect is not convincing. The feeling that either the Laurentian aspect or the Brewerton focus must be placed in *either* an Archaic or Woodland pattern is to be deplored. One of the interesting sidelights is the evidence given on pages 241 and 259 for the use of copper implements in New York before the arrival of pottery. Ritchie's interpretation of the Robinson site as one showing "cultural continuity with change" is one which he could profitably adopt for the entire archaeological picture. The suggestion is offered that Vergennes may be later than Brewerton and that it participated more closely in the culture type or period to which the "Red Paint culture" belongs. What the "Laurentian aspect" apparently needs is a considerable amount of spade work north and east of New York, with a careful presentation of culturally related items rather than comparisons with surface collections and assumed complexes from local areas.

The Frontenac Island site is an absorbing one and Ritchie's interpretation of the cultural association may well be sound. One can but wonder about the sudden increase in burial furniture in the Frontenac focus when it is usually absent in other foci attributed to the Archaic pattern but is a common trait in the intermediate cultures.

The Lamoka focus in its present status is an orphan in the northeast. As Ritchie indicates on page 292, stratigraphic proof of its priority over Laurentian is lacking. Some

students are worried about the presence of refuse pits and milling stones at a site called pre-agricultural. These are not the only resemblances between Lamoka and the San Pedro stage of Cochise.

Various problems which require additional work are discussed in the last few pages before the comparison and conclusions. Here the fluted points, weathered argillite points, and the Perch Lake Mounds are considered, and there is a recapitulation of the problems which Ritchie recognized earlier in the volume and some of which have also been mentioned in this review. The final paragraph of page 318 is in itself the most satisfactory evidence of Ritchie's competence as an archaeologist. No one will examine the cultural arrangement as presented in this work with a more critical eye than will Ritchie, and I am confident that he himself will provide many changes in the next twenty years.

The usefulness of the graphs on Plate 165 is somewhat impaired by the method of reproduction and by the attempt to place too much data on one sheet. If the horizontal columns for the specifically mortuary complexes of the intermediate period were removed, a more accurate arrangement of the cultural change through time in New York would result. The trait tables prepared by Ritchie which appear on pages 329–394 contain much of the data which he used to create his classificatory divisions and on which he based his three major time horizons. These tables could result only from a considerable expenditure of effort and no one can be said to fully understand the volume who has not mastered the data presented therein. The reviewer has not done this and is open to the accusation of commenting upon problems which the author is much more competent to handle and with which he is much more familiar.

On pages 319–320, Ritchie discusses the interpretation of the graphs and recognizes that, "a thread of progression is, for the most part, traceable throughout." This is perhaps the first time that the point of view of studying the NewYork sequence as a developmental continuum has been presented. This appears to the reviewer to be not a "thread" but a main line of approach. There are at least two kinds of archaeological interpretations. In the first, the area or site being reported upon is viewed as the culture center and point of dispersal for almost all of the elements found therein. The archaeological maxim in operation is that "the other man's area is always marginal and therefore later." Such presentation is sometimes accompanied by a slipshod definition of culture complexes and for this there are sometimes substituted developmental charts on sliding scales permitting rapid movement of traits in the most convenient temporal direction. In the second method of presentation, the area under consideration is viewed as a marginal one and a cultural sink for all the surrounding regions, thus absolving the reporter from the necessity of explaining the growth and development in his area except by the convenient method of group or cultural movement.

Ritchie's presentation of his culture groups is in the form of an objective description of the data on the basis of which he has placed the various cultural divisions within a taxonomic framework. This method of presentation was followed because of Ritchie's participation in the archaeological conferences of midwestern students during the last decade. During the 1920's and particularly during the 1930's, there was increasing realization among archaeologists of the eastern United States of a need for the recognition and definition of the various cultural units within the area. The difficulties involved in ordering the heterogeneous data throughout the region made them welcome a taxonomic framework. This framework, which goes under a number of published and unpublished names, has been of value in that it requires a considerable effort on the part of archaeologists to prepare their reports in such a manner that they demonstrate the cultural relationship suggested. Furthermore, the discussions which arose as a result of the attempt to apply this methodology focused attention as never before on all possible aids in interpreting the prehistory of the area. This resulted in a fairly uniform method of

attack on common problems and, in the reviewer's opinion, has had a beneficial effect primarily from the standpoint of analysis and typology. One of the unfortunate results of some of the applications of the method has been the emphasis placed on traits which serve to differentiate any unit from all other units. When this emphasis results in slighting the traits which connect units—and particularly temporally successive units—one of the major aims of archaeology is defeated. This tendency is particularly unfortunate when sites which are transitional from one culture group to another and from one type of subsistence to another are dealt with. The classificatory approach in such cases has often resulted in ignoring the very items which are the most important in understanding the story of the cultural development of the people who occupied the site.

The taxonomic and classificatory emphasis has served a useful purpose, but the well-recognized succession of cultures throughout the eastern United States invites the abandonment of attempts to complete the arbitrary taxonomic framework. The time and effort expended in trying to determine the elements held in common throughout the Woodland pattern (which is a valuable abstraction but not an historical entity) would have been much better used in an attempt to discover the sequence of, and continuity within, the various units recognized in their own areas.

Ritchie is to be congratulated upon the successful completion of this notable monograph, which has been awarded an A. Cressy Morrison Prize in Natural Science and which was accepted as his Ph.D. thesis at Columbia University. The volume not only marks a significant point in Ritchie's career but it is a product which will aid both him and his colleagues in the continuing effort to interpret the cultural history of the Northeast. It can be viewed as almost a "necessary" step in providing the skeletal outline upon which future interpretations will lean for the data to substantiate theories regarding local cultural evolution and for "ethnological" reconstructions of the life of the people.

JAMES B. GRIFFIN
University Museums
Ann Arbor, Michigan

Archaeological Regions of Colombia: A Ceramic Survey. WENDELL C. BENNETT. (Yale University Publications in Anthropology, No. 30, Yale University Press, New Haven. 120 pp., 26 text figs., 9 tables, 12 pls.)

Excavations in the Vicinity of Cali, Colombia. JAMES A. FORD. (Same series, No. 31. 82 pp., 19 text figs., 4 pls.)

In western Colombia the northern end of the great Andean mountain chain splits up into four lesser ranges which run, parallel to each other, north toward the Isthmus and the Caribbean. They are separated by major river valleys and are bounded on the west by the Pacific coast and on the east by the llanos of the Orinoco. In considering the interrelationship of native Central and South American cultures, this northern fringe of the vast Andean area occupies a strategic position. The Colombian mountain systems and their intermontane valleys provide the most likely land passage between the continents. Colombia, whose prehistory has been little clarified, constitutes, along with Ecuador and southern Central America, an unknown intermediate area which makes it impossible to cross-date the well-known Middle American and Peruvian sequences.

In 1941–1942, Bennett and Ford conducted archaeological explorations in one of the Colombian intermontane valleys and made a general survey and recapitulation of Colombian archaeology. The two papers reviewed are a report of this work, which was undertaken as a part of the program of the Institute of Andean Research. Bennett's paper is without doubt the best, most concise, and most up-to-date summary of Colombian archaeology yet published. It is based upon the 1941–1942 field work, upon examina-

tion of private and public collections in Colombia and in the United States, and upon existing literature. As such, it is a necessary manual for anyone interested in South American archaeology. Ford's report is a detailed account of field operations in the upper Cauca valley.

Bennett and Ford began their work with the purpose of gathering new information that would help solve the problems of inter-Andean and intercontinental cultural relationships. In keeping with this aim, they selected a little-known region around Cali in the fertile upper Cauca for intensive work. This section seemed well chosen in terms of the problem, but, in spite of this, they were unsuccessful in their attempt to find specific clues to cultural relationships. This appears to have been the fault of no one except, possibly, the former Indian inhabitants of the country; Ford's Cali survey, to judge from his report, was painstaking and thorough, even if results did not come up to expectations. Nevertheless, the total effect of the Bennett-Ford data will not be unimportant in the solution of such major problems.

In brief, Ford was able to define three ceramic complexes in the upper Cauca: the Río Pichindé, the Río Bolo, and the Quebrada Seca. Although direct evidence was lacking, he feels that they are all quite late—just pre-Conquest. These ceramic complexes, though not highly distinctive, are, nonetheless, decidedly localized styles. Their complete distribution is as yet unknown, but it can be said without hesitation that they do not appear to be closely related to Peruvian, Ecuadorean, other local Colombian, or Central American styles. Virtually all of the upper Cauca pottery came from shaft graves. These graves were located on the slopes of the valley walls and on the tops of mountain ridges. House platforms, around which there was very little cultural refuse, were usually situated nearby. The most interesting feature of the work was the failure to find concentrated villages or communities. The ancient inhabitants of the Cauca seem to have been hill-slope, milpa farmers, widely scattered in isolated houses.

Bennett, in his summary, discusses Colombian archaeology in terms of nine archaeological regions. Although present information on the country is very uneven, it is fairly clear that cultural regionalism was marked in prehistoric times. The regions—San Agustín, Tierradentro, Nariño, Upper Cauca, Quimbaya, Sinu, Chibcha, Mosquito, and Santa Marta (Tairona)—are each characterized by distinctive cultures. There is a little, but not much, evidence of specific trade contacts and interchange among these regions. Some of this regional diversity can probably be explained by time differences. Bennett is inclined to think so, and he presents a tentative chronological arrangement of Colombian archaeological regions and, in some places, complexes within regions. The arrangements are admittedly based upon fragmentary and indirect evidence. In main outline, Upper Cauca, Chibcha, and Santa Marta are placed in a late period; Quimbaya and Nariño, in a middle period; and San Agustín and Tierradentro, in an early period. Even if one attributes some of the cultural variation to time differences he still has the feeling that the Colombian archaeological picture is one of a number of old localized traditions which have persisted, more or less undisturbed, from early to late times.

Bennett's projected culture sequence does, however, seem to explain some of the discrepancies in trait distributions for the different regions. For example, although Nariño three-color negative-painted pottery is not stylistically the same as Quimbaya, the presence of this technique of painting in three colors suggests a relationship. The fact that three-color negative is absent from the intervening San Augustín and Tierradentro regions does not distort the distributional picture when the latter cultures are placed as earlier than Nariño and Quimbaya, at a period when three-color negative was not an influence in Colombia. Roughly, Bennett attempts to correlate Colombia with the Peruvian and Ecuadorean sequences, and the negative-painting technique is one datum for these correlations. Recuay, Tuncahuán, and Nariño are placed on a general

post-Chavín but pre-Tiahuanacoid level. Quimbaya is also equated on this level. In Quimbaya, such Peruvian pottery traits as modeling, double bowls, double spouts with flat bridge, stirrup spouts, and white-on-red painting do not belong specifically to any one Peruvian period or style, although they are, for the most part, post-Chavín. Like three-color negative, these features are missing from Tierradentro and San Agustín.

This Recuay-Quimbaya correlation implies a gross Chavín-San Agustín contemporaneity, which may be the case. San Agustín is certainly the only Colombian culture in which massive stone carving has been recorded, and, although Chavín-San Agustín similarities of style are virtually absent, there is a suggestion in San Augustín of generic relationship to central and south Andean stone sculpture as a whole. The same, however, could be said for San Agustín similarities to the non-Mayan stone sculpture of Central America.

So far, the reviewer has presented only a synopsis of the framework of conditions and problems within which the authors have treated their data. In his opinion, the two papers will stand on their merits as descriptive reports, alone. As to the problem of migrations and cultural connections between Middle and South America, the Bennett-Ford contributions are, admittedly, negative. It is here that certain questions might be raised. Ford and Bennett both flatly state that their work has added nothing new toward the solution of these problems. In this way it is implied, although not said, that a very special type of evidence must be forthcoming before inter-American or widespread Andean relationships can be satisfactorily demonstrated. Presumably, nothing less than specific style similarities, in ceramics or other manufactures, would be acceptable as proof of linkage. In the absence of such proof, one is left with the conclusion that the choice of the upper Cauca for intensive excavations was simply a stroke of bad luck and that connections of a stylistically unmistakable sort are yet to be found in some other locality—such as the Magdalena Valley or the Pacific coast. Logically, then, this would mean that, if such definitive proof is never uncovered elsewhere, we must conclude that Middle and South America did not have interrelated histories, at least in their later stages of development. It is not likely that either author would go this far or deny diffusion between the two continents. What is actually missing in Bennett and Ford's data, then, is not evidence of relationship between Central and South America but clear-cut style and type patterns by which the Peruvian and Middle American sequences could be correlated period for period.

The possibility should be faced that cultural connections between Central and South America may never be demonstrated on the level of style specificity, because such evidence may not exist. It is quite possible that evidence of this type is an archaeological hope which has no reality and that cultural ideas were transmitted in a way which would not leave traces of such an obvious sort. The intense regionalism manifested in lack of cultural similarity between relatively restricted adjacent regions within Colombia, Ecuador, and Peru, between Peru and Ecuador, or between Ecuador and Colombia is an argument in support of this thesis. If style and type cross-overs from one locale to another all down the Andes are difficult to isolate, it is reasonable to suppose that cross-overs from Peru to Middle America via Colombia will be equally, if not more, difficult to find. In line with these arguments, it should be stated that even in Peru there is marked regionalism, although this has been somewhat obscured by the unifying sweep, at three different times, of highly individualized art styles. These horizon styles have been the means of successfully linking Peruvian regional sequences. Were Bennett and Ford expecting to find something comparable to a horizon style as a means of solving the problems of wider geographical connections with Central America? Although such specific style similarities between Middle and South America have not been, and may never be, demonstrated, there is a common participation in a number of techniques and

basic ideas which suggests relationship. Negative painting and complicated pottery vessel forms are examples. Bennett has used this type of data, without placing too much confidence in it, in his tentative Peru-Ecuador-Colombia equations, but he has not suggested its possible validity for correlations in the other direction.

It has been a commonly accepted idea, to which the reviewer has also subscribed, that generic similarities of a technological rather than a stylistic order, found in widely separated areas of the Americas, may derive out of an early and as yet undisclosed inter-American substratum from which subsequent regional specializations developed. This remains as a possible explanation, although it is one of the archaeologist's overworked stock-in-trade concepts. However, it seems more likely that diffusion of technological processes increased with growing regional specialization and complexity, rather than ceasing or diminishing. It is interesting that certain techniques, such as negative painting, do not occur in the earliest periods but first appear later in the sequences in both south and Central America, fairly well up in the time scales which are available to the archaeologist.

Apparently some of the craft techniques and ideas common to the higher American cultures were transmitted long after the original spread and development of the initial horticultural stages, yet these transmissions left little in the way of specific stylistic clues. Probably more often than not the passage of techniques from one region to another was accomplished without breaking up, or seriously modifying, regional styles which had become firmly set in the native locale. It may eventually be shown that, lacking horizon style markers with which to correlate South and Central America, we will have to re-examine and regroup data into categories on a more general level than that of "style" or "type." What has been accepted as only suggestive and tentative may have to carry the burden of proof.

GORDON R. WILLEY
Bureau of American Ethnology
Smithsonian Institution
Washington, D.C.
September, 1944

The University of New Mexico and the Laboratory of Anthropology announce joint publication of the quarterly *Southwestern Journal of Anthropology*, a periodical devoted to the general field of anthropology. Plans call for a volume of 400 to 600 pages. Subscriptions, at $4.00 a year, should be addressed to the University of New Mexico Press, Albuquerque, New Mexico.

Contribution of manuscripts is invited by the Editor, Leslie Spier, University of New Mexico, Albuquerque, New Mexico.

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GENERAL

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HOWELLS, LT. W. W., 2432 Tracy Place, N.W., Washington 8, D.C.
KRAFT, J. L., 400 Rush Street, Chicago, Ill.
LLOYD, MALCOLM, JR., 1930 Land Title Building, Philadelphia 10, Pa.
NELSON, N. C., American Museum of Natural History, New York 24, N.Y.
PARK, DR. W. S., 560 Charleston Boulevard, Las Vegas, Nev.
TRUMBULL, MRS. WALTER, 75 Central Park West, New York 23, N.Y.
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ALBERTSON, SGT. WILLIAM E., 18049248, 2515th AAFBU, Sec. B, Dodge City, Kans.
ALBRECHT, MR. and MRS. HERBERT, 62 S. Hillcrest Road, Springfield, Delaware County, Pa.
ALDRIDGE, C. CLAY, 26 Bellaire Drive, Montclair, N.J.
ALEXANDER, H. G., University of New Mexico, Albuquerque, N. Mex.
ALLEN, HUGH S., 2307 Cedar Street, Bakersfield, Calif.
ALLEN, R. E., 1107 S. Gramercy Place 6, Los Angeles 6, Calif.
ALLEN, MRS. W. E., Route 7, Box 69, Fort Worth, Tex.
ALLYN, HARRIETT M., Mount Holyoke College, South Hadley, Mass.
ANDERSON, ARTHUR J. O., Eastern New Mexico College, Portales, N.M.
ANDREWS, E. WYLLYS, 10 Frisbie Place, Cambridge 38, Mass.
ANTEVS, ERNST, The Corral, Globe, Ariz.
ANTLE, H. R., 636 Amarillo Building, Amarillo, Tex.
ARNOLD, SGT. BRIGHAM A., 38070694, H. & S. Co., 316 Engr. Combat. Bn., A.P.O. 91, c/o Postmaster, New York, N.Y.
ARNOLD, WAYNE S., 38 Church Street, Little Falls, N.Y.

BABBITT, J. R., 519 N. Leroux Street, Flagstaff, Ariz.
BABCOCK, WILLOUGHBY M., Minnesota Historical Society, St. Paul, Minn.
BABY, RAYMOND S., Dept. of Anatomy, Western Reserve University, 2109 Adelbert Rd., Cleveland 6, Ohio
BAERREIS, DAVID A., 1008 Monnett Avenue, Norman, Okla.
BAGGERLY, CARMEN, 1900 Brookwood Avenue, Toledo 2, Ohio
BAILEY, JOHN H., Davenport Public Museum, 704 Brady St., Davenport, Iowa
BAILEY, WILFRID C., Box 82, Pomerene, Ariz.
BAKER, WILLIAM E., Boise City, Okla.
BALDWIN, GORDON C., 639 California Avenue, Boulder City, Nev.
BARCLAY, GEORGE C., Box 314, Newport News, Va.
BARNETT, H. G., Smithsonian Institution, Bureau of Americau Ethnology, Washington 25, D.C.
BARTLETT, KATHERINE, Box 601, Museum of Northern Arizona, Flagstaff, Ariz.
BASTO, ARTHUR E., Gossville, N.H.
BEALS, RALPH L., Department of Anthropology and Sociology, University of California, Los Angeles, Calif.
BEARD, ROLAND E., Edna, Tex.
BEARDSLEY, RICHARD K., c/o Professor Jacob Cornog, 1155 E. Court Street, Iowa City, Iowa
BEAVER, W. H. T., 2951 Hermosita Drive, Glendale 8, Calif.
BELL, ROBERT E., 531 North Main Street, Marion, Ohio
BENEWAY, ALMON B., Vassar View Road, Poughkeepsie, N.Y.
BENGEL, W. H., Newport, Ark.
BENNETT, JOHN W., Department of Sociology, Ohio State University, Columbus 10, Ohio
BENNETT, WENDELL C., 55 Hillhouse Avenue, New Haven 11, Conn.
BERLIN, HEINRICH, Apartado 1879, Mexico, D.F.
BERNE, ALBERT, Box 458, Madeira, Ohio
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BILGERY, CONRAD, Regis College, W. 50th and Lowell Blvd., Denver 11, Colo.
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BLAKE, LEONARD W., 5784 Pershing Avenue, St. Louis, Mo.
BLUMENTHAL, E. H., JR., 802 Armijo Road, Albuquerque, N.M.
BOGAN, SAMUEL D., 106 Bedford Avenue, Hamden, Conn.
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BRAME, J. Y., Box 1366, Montgomery 2, Ala.
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*BRITTEN, MRS. MARION HALE, Division of Anthropology and Psychology, National Research Council, Washington, D.C.
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*CASO, ALFONSO, Instituto Nacional de Antropología é Historia, Zacatecas No. 218, Mexico, D.F.
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COLE, FAY COOPER, Department of Anthropology, University of Chicago, Chicago 37, Ill.
COLEMAN, LEE L., Lamar Ranger Station, Via Gardiner, Mont.
COLLINS, HENRY B., JR., Bureau of American Ethnology, Smithsonian Institution, Washington 25, D.C.
COLLINS, KINS, 757 Westchester, Grosse Pointe 30, Mich.
COLTON, HAROLD S., Box 601, Museum of Northern Arizona, Flagstaff, Ariz.
CONGDON, DR. WM. J., 611 Prince Henry Avenue, Hopewell, Va.
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COOPER, PAUL, 509 West 121st Street, Apartment 212, New York 27, N.Y.
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CORDY, N., 548 E. Avenue 39, Los Angeles 31, Calif.
COSGROVE, MRS. HARRIET S., Peabody Museum, Harvard University, Cambridge 38, Mass.
COY, EDNA, Route 3, Box 132B, Amarillo, Tex.
CRESSMAN, L. S., University of Oregon, Eugene, Ore.
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DANE, S/SGT. WALTER P., 1131 Harmar Street, Fort Wayne, Ind.
DANIELS, MRS. HELEN S., Box 1468, Durango, Colo.
DANSON, EDWARD B., Kenyon Ranch Tubac, Box 208, Tucson Nogales Star Route, Nogales, Ariz.
DAVIDSON, D. S., Department of Anthropology, University of Pennsylvania, Philadelphia, Pa.
DAVIDSON, J. M., Griffith Lodge, Gartcosh, Lanarkshire, Scotland
DAVIS, E. MOTT, JR., Shirley, Mass.
DAVIS, H. E., Hotel Pawnee, North Platte, Neb.
DAVIS, HARRY T., North Carolina State Museum, Raleigh, N.C.
DAVIS, WALTER M., 1140 W. Brower Street, Springfield, Mo.
DAY, H. SUMMERFIELD, 1442 Algonquin Road, Des Plaines, Ill.
*HERNANDEZ DE ALBA, GREGORIO, Apartado 23 34, Bogota, Colombia, S.A.
DEAN, LESLIE V., Hollister, Idaho
DEEVEY, EDWARD S., JR., Woods Hole Oceanographic Institution, Woods Hole, Mass.
DE LAGUNA, FREDERICA, 221 Roberts Road, Bryn Mawr, Pa.
DELAVAN, WAYNE, Rural Route 2, Box 61, Bronson, Kans.
DELLINGER, S. C., University of Arkansas, Fayetteville, Ark.
DENISON, MRS. JOHN H., JR., Sheridan County, Big Horn, Wyo.
DEUEL, THORNE, Illinois State Museum, Springfield, Ill.
DICK, HERBERT L., 243 N. Hermosa Avenue, Albuquerque, N.M.
DICKEY, MRS. PARKE A., 194 Interstate Parkway, Bradford, Pa.
DISINGER, FOSTER, 126 Front Street, Binghamton, N.Y.
DODGE, V. K., 137 Bell Court West, Lexington, Ky.
DOERSCHUK, HERBERT M., Box 505, Badin, N.C.
DOUGLAS, FREDERIC H., 1300 Logan Street, Denver, Colo.
DOYLE, MRS. JOHN L., JR., 927 E. Highland Avenue, Redlands, Calif.
DRAKE, PFC. ROBERT J., 19086307, Hq. 2nd Prov. T.C. Group, Pope Field, Fort Bragg, N.C.
DUANE, WM. NORTH, P.O. Box 989, Santa Barbara, Calif.

DUFFEN, WILLIAM A., Temple Court Apartments No. 25, Baltimore 18, Md.
DUSTIN, FRED, 705 S. Fayette Street, Saginaw, W.S. 5, Mich.
DUTTON, BERTHA P., Museum of New Mexico, Santa Fe, N.M.

EBERHARDT, HENRY J., 69 Prospect Street, Newark 5, N.J.
EDMUNDSEN, W. T., 1125 S. 7th, Hood River, Ore.
EDWARDS, H. ARDEN, Antelope Valley Indian Research Museum, Wilsona Route, Lancaster, Calif.
EICHENBERGER, J. ALLEN, 523-A N. Fifth Street, Hannibal, Mo.
EISELEY, LOREN C., Department of Sociology, Oberlin College, Oberlin, Ohio
EKBLAW, W. ELMER, Clark University, Worcester 3, Mass.
EKHOLM, GORDON F., American Museum of Natural History, Central Park West at 79th Street, New York 24, N.Y.
ELLIS, H. HOLMES, 212 Hampton S.E., Grand Rapids, Mich.
ELLSWORTH, B. E., 302 S. Maple, North Platte, Neb.
ENGERRAND, GEORGE C., Department of Anthropology, University of Texas, Austin, Tex.
ERWIN, HENRY P., 723 15th Street, N.W., Washington 5, D.C.
ESKEW, JAMES W., Findlay, Ill.
EVANS, CLIFFORD, 8711 La Tuna Canyon Road, Roscoe, Calif.
EVANS, MRS. GERTRUDE S., 350 S. Main Street, Freeport, N.Y.
EVER, NEOLA A., 50 East Pierson Street, Phoenix, Ariz.

FAIRBANKS, CHARLES H., 657 College St., Macon, Ga.
FALKENBACH, CHARLES H., American Museum of Natural History, 77th St., and 8th Ave., New York 24, N.Y.
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FARVER, S. S., 37 N. Railroad Street, Palmyra, Pa.
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FENENGA, FRANKLIN, 3719 Blow Street, St. Louis 16, Mo.
FENTON, WILLIAM N., Bureau of American Ethnology, Smithsonian Institution, Washington 25, D.C.
FERGUSON, MRS. ALICE L. L., 2330 California Street, N.W., Washington 8, D.C.
FINLEY, O. M., Rock Springs, Wyo.
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FORD, JAMES A., 1808 Cloverdale Street, University Gardens, Baton Rouge, La.
FOSTER, CAPT. JAMES R., Springfield, Ky.
FOWLER, WILLIAM S., 118 Central Park Drive, Holyoke, Mass.
FRANKE, WALTER E., 198 East Street, North Attleboro, Mass.
FRANZ, HARVEY, 1027 Ogden Avenue, Benton Harbor, Mich.
FRASER, DOROTHY E., 34-33 90th Street, Apartment F63, Jackson Heights, N.Y.
FRENCH, W. C., 1018 West Street, Grinnell, Iowa
FULLER, CHARLES V., Grand Ledge, Mich.

GABEL, LT. NORMAN E., 4123 University Station, Tucson, Ariz.
GARFAT, RUSS C., Box 223, Cabri, Saskatchewan, Canada
GARRETT, KENTON, 1807 Carmelina Drive, San Diego 3, Calif.
GARTH, THOMAS R., JR., 1036 Key Route Boulevard, Albany 6, Calif.
GAVAN, JAMES A., 3121 S. Taylor Avenue, Milwaukee 7, Wis.
GAYTON, ANNA N., P.O. Box 880, Santa Cruz, Calif.
GEBHARD, PAUL H., 5 West Gate, 11 Everett Street, Cambridge, Mass.
GENTLES, JOHN, 1321 Garden St., Glendale, Calif.
GESSNER, GEORGE N., 11 Adams Street, Dansville, N.Y.
GETTY, HARRY T., Arizona State Museum, Tucson, Ariz.
GIDDINGS, LT. (j.g.) JAMES L., JR., Route 5, Box 770, Tucson, Ariz.
GIFFORD, E. W., Anthropology Museum, University of California, Berkeley, Calif.
GIFFORD, STANLEY, 946 Euclid Avenue, Syracuse 10, N.Y.
GILKYSON, MRS. PHOEBE H., Mont Clare, Montgomery County, Pa.
GILMORE, DR. G. H., Murray, Neb.
GILPIN, LAURA, 334 N. Belmont, Wichita 8, Kans.
GINNANE, E. S., 1016 Martin Building, Birmingham, Ala.
GIRARD, RAFAEL, Corquin, Departamento de Copan, Honduras
GLADWIN, HAROLD S., P.O. Box 540, Santa Barbara, Calif.
GODDARD, CHARLES F., Mattituck, Long Island, N.Y.
GODFREY, MRS. WILLIAM S., Ardmore, Pa.
GOGGIN, JOHN M., Peabody Museum of Natural History, Yale University, New Haven, Conn.
GOODWIN, POWELL, 3504 Grooms Street, Austin 21, Tex.
GOSLIN, ROBERT M., 316 Wilson Avenue, Columbus 5, Ohio
GREEN, AMOS R., R.R.1, Eau Claire, Mich.
GREENEBAUM, MILTON L., 308 Jefferson Apartments, Saginaw, Mich.
GREENMAN, EMERSON F., University Museums Building, Ann Arbor, Mich.
GRIFFIN, JAMES B., University Museums Building, Ann Arbor, Mich.
GRIFFIN, JOHN W., 309 N. Grandview Avenue, Daytona Beach, Fla.
GRIGGS, EVERETT, Abington, Conn.
GRIMM, SGT. ROBERT E., 305 East 4th Street, O'Fallon, Ill.
GRINDELL, J. C., 2 Garden Lane, Kirkwood, Mo.
GUERNSEY, E. Y., 1504 Fourteenth Street, Bedford, Ind.
GUTHE, CARL E., New York State Museum, State Education Department, Albany 1, N.Y.

HAAG, LT. WILLIAM G., Fair Haven Road, Rumson, N.J.
HACKETT, DR. MARTHA, 4937 Pasadena Avenue Terrace, Los Angeles 42, Calif.
HADLOCK, WENDELL S., Water Street, Ellsworth, Me.
HALL, MILTON B., 85 Grosvenor Avenue, East Providence 14, R.I.
HALL, WILL L., 6219 Rosebury Avenue, St. Louis, Mo.
HALLETT, LEAMAN F., 31 West Street, Mansfield, Mass.
HALSETH, ODD S., Route 9, Box 425 E, Phoenix, Ariz.
HAMILTON, HENRY W., 787 Eastwood, Marshall, Mo.
HANSEN, BURTON D., 1941 23rd Street A., Moline, Ill.
HARRINGTON, M. R., The Southwest Museum, Highland Park, Los Angeles 42, Calif.
HARRIS, ALFRED, II, Department of Anthropology, University of Chicago, Chicago 37, Ill.
HARROLD, C. C., 700 Spring Street, Macon, Ga.
HART, GEORGE E., Wading River, Long Island, N.Y.

HAUBERG, JOHN, 23rd Street and 13th Avenue, Rock Island, Ill.
HAURY, EMIL W., Department of Anthropology, University of Arizona, Tucson, Ariz.
HAWLEY, F. G., Box 351, Miami, Ariz.
HEDGECOKE, E. V., Box 11, Stinnett, Tex.
HEIZER, ROBERT F., Dept. of Anthropology, University of California, Berkeley 4, Calif.
HENCKEN, H. O'NEIL, Peabody Museum, Harvard University, Cambridge 38, Mass.
HENDRY, J. R., 8167 Lasalle Boulevard, Detroit, Mich.
HENRY, JAMES F. B., 34 Park Street, New Haven, Conn.
HERSKOVITS, M. J., Northwestern University, Evanston, Ill.
HERTZBERG, EDWARD, 333 E. Mosholu Parkway, New York, N.Y.
HEWES, GORDON W., 1639 Wisconsin Avenue, N.W., Washington 7, D.C.
HIBBEN, FRANK C., 129 Leland Street, Chevy Chase, Md.
HILL, A. T., P.O. Box 100, Capitol Building, Lincoln 9, Neb.
HILL, GERTRUDE, Flagstaff Public Library, Flagstaff, Ariz.
HILL, JAMES A., 99 Tracy Avenue, East Lynn, Mass.
HILL, W. W., Department of Anthropology, University of New Mexico, Albuquerque, N.M.
HODGE, F. W., The Southwest Museum, Highland Park, Los Angeles 42, Calif.
HODGES, DR. T. L., Bismarck, Ark.
HOFMANN, ARTHUR M., Ballardvale, Mass.
HOLLADAY, JAMES A., 16 Beach Drive, Norotón, Conn.
HOLLAND, C. G., 1618 Oxford Road, Charlottesville, Va.
HOLLENBACH, MARION, Los Angeles County Museum, Exposition Park, Los Angeles 7 Calif.
HOLMQUIST, CARL E., 441 Parkdale Avenue, Royal Oak, Mich.
HOOD, RAYMOND S., Box 486, Tonopah, Nev.
HORNBLOWER, MAJ. RALPH, JR., 7 Louisburg Square, Boston, Mass.
HORNER, GEORGE R., 871 Clifton Avenue, Newark, N.J.
HOUSEHOLDER, J. C., 3957 College Avenue, Indianapolis, Ind.
HOWARD, GEORGE D., Peabody Museum, Yale University, New Haven, Conn.
HOWARD, ROBERT R., 503 South 6th Street E., Missoula, Mont.
HOWE, BRUCE, 1821 H. Street, N.W., Washington 6, D.C.
HOWE, J. V., Jeffress, Va.
HOWES, WILLIAM J., 37 Harvard Street, Holyoke, Mass.
HUCKSOLL, MRS. LAWRENCE L., 644 4th Street, Portsmouth, Ohio
HUGHES, ENS. J. T., U.S.N.R., USS Vega, c/o Fleet Post Office, San Francisco, Calif.
HULBERT, MILTON, F., Lock Box No. 35, Reedsburg, Wis.
HULSE, FREDERICK S., 5916 16th Street North, Arlington, Va.
HURST, C. T., Western State College, Gunnison, Colo.
HURT, CPL. WESLEY R., 78th Infantry Division, Camp Pickett, Va.

IHNEN, W. B., 1459 Oriole Drive, Los Angeles 46, Calif.
IRVINE, SAM C., Marshall, Mo.

JACKSON, GEORGE, R.F.D. 1, Sandy Hook, Conn.
JAKEMAN, M. WELLS, 1232 W. 4th Street, Los Angeles, Calif.
JENKINS, J. T., 1335 23rd Street, Newport News, Va.
JENNESS, D., National Museum of Canada, Ottawa, Ontario, Canada
JENNINGS, LT. J. D., Box 428, Tupelo, Miss.
JENSEN, DOROTHY CROSS, Fallsington, Pa.
JERRARD, L. P., 522 Willow Avenue, Winnetka, Ill.
JOHNSON, FREDERICK, R. S. Peabody Foundation, Phillips Academy, Andover, Mass.
JOHNSON, LESLIE R., 90 Concord Avenue, Glen Rock, N.J.
JOHNSTON, LA DOW, 2435 Copland Boulevard, Toledo 9, Ohio
JOHNSTON, RUSSELL A., Helmsdale Post Office, Alberta, Canada
JONES, HOWARD A., 70 Greenwood Avenue, Greenwood, Mass.
JONES, VOLNEY H., Museum Building, Ann Arbor, Mich.
JONES, WALTER B., Alabama Museum of Natural History, University of Alabama, University, Ala.
JUDD, NEIL M., U. S. National Museum, Washington 25, D.C.
JURY, WILFRID, University of Western Ontario, London, Ontario, Canada

KAISER, LT. ALBERT DAVID, O 1041806, Officers Mail Section, Army Post Office BTC 10, Greensboro, N. Car.
KAMPERMAN, DR. GEORGE, 79 Longfellow Avenue, Detroit, Mich.
KAPLAN, BERNICE, 105 West 72nd Street, New York 23, N.Y.
KASTNER, DR. A. L., 1810 W. Wisconsin Avenue, Apartment 308, Milwaukee 3, Wis.
KAUFFMAN, CHARLES F., 826 Florida Avenue, York, Pa.
KAUFMAN, LOUIS, 835 Westholme Avenue, West Los Angeles 24, Calif.
KAUTZ, HAROLD M., 3253 Sondra Drive, Fort Worth, Tex.
KAVANAUGH, DR. CHARLES N., 190 N. Upper Street, Lexington, Ky.
KEITH, MRS. E. GORDON, 607 Whittier Street, Washington 12, D.C.
KELEMEN, PAL, Norfolk, Conn.
KELLY, F. A., 30 66th Place, Long Beach 3, Calif.
KELLY, ISABEL, c/o American Consulate, Guadalajara, Jalisco, Mexico
KELLY, W. H., Peabody Museum, Harvard University, Cambridge 38, Mass.
KEMPTON, DR. ROCKWELL M., 333 S. Jefferson Avenue, Saginaw, Mich.
KENT, MRS. EDWARD G., 39 Greenvillage Road, Madison, N.J.
KENT, KATE PECK, c/o A. S. Peck, 2115 East 14th Avenue, Denver 6, Colo.
KEUR, MRS. DOROTHY L., 2475 Palisade Avenue, Spuyten Duyvil, New York 63, N.Y.
KEYES, CHARLES R., 323 Tenth Avenue S., Mt. Vernon, Iowa
KIDD, KENNETH E., 100 Queens Park, Toronto, Ontario, Canada
KIDDER, A. V., 10 Frisbie Place, Cambridge 38, Mass.
KIDDER, ALFRED II, Peabody Museum, Harvard University, Cambridge 38, Mass.
KING, ARDEN, Department of Anthropology, University of California, Berkeley 4, Calif.
KINGMAN, MRS. EUGENE, 2517 K Street, N.W., Apartment 501, Washington 7, D.C.
KIVETT, S/SGT. MARVIN F., 1328 K Street, Lincoln, Nebr.
KLUCKHOHN, CLYDE, Peabody Museum, Harvard University, Cambridge 38, Mass.
KNEBERG, MADELINE, West Strong Hall, University of Tennessee, Knoxville, Tenn.
KNECHTEL, FRITZ, Hanover, Ontario, Canada
KNOWLES, LT. COL. N., 2118 Tunlaw Road, N.W., Washington 7, D.C.
KNOX, MRS. MERTICE M. C. BUCK, Box 17, Kelvin, Ariz.
KRETSCHMER, EDWARD E., 5821 W. Walton Street, Chicago, Ill.

KRIEGER, ALEX D., 601 East 19th Street, Austin 22, Tex.
KROEBER, A. L., University of California, Berkeley 4, Calif.
KROHN, ERNST C., 3806 Juniata Street, St. Louis, Mo.

LANGE, CHARLES H., JR., 5142 MacArthur Boulevard, Washington, D.C.
LANGFORD, GEORGE, 1506 Hawthorne Drive, R.D. 3, Joliet, Ill.
LANGFORD, R. R., 303 West 4th Street, North Platte, Nebr.
LARU, H. C., Rural Route 1, Chariton, Iowa
LATHAM, ROY, Orient, Long Island, N. Y.
LAUGHLIN, WILLIAM S., 1705 Court Street, Salem, Ore.
LEECHMAN, DOUGLAS, National Museum, Ottawa, Ontario, Canada
LEH, L. L., 3365 4th Street, Boulder, Colo.
LEONTINE, DR. G. E., 31 Waldeck Street, Dorchester, Mass.
LEWIS, KEPLER, 8931 Yorktown Avenue, Los Angeles 45, Calif.
LEWIS, MARY BUTLER, Media, Pa.
LEWIS, OSCAR T., P.O. Box 464, Billings, Mont.
LEWIS, T. M. N., University of Tennessee, Knoxville, Tenn.
LIGHT, MRS. GEORGE P., 1012 Tombstone Canyon, Bisbee, Ariz.
LILLY, ELI, 5807 Sunset Lane, Indianapolis 5, Ind.
LINDSLEY, GEORGE M., Veblen, S. Dak.
LINTON, RALPH, Department of Anthropology, Columbia University, New York 27, N.Y.
LISTER, CAPT. ROBERT, 111 N. Virgil, Los Angeles 4, Calif.
LOHR, EDISON P., 1506 Cleveland Avenue, Loveland, Colo.
LONGDEN, GRAFTON J., JR., Rural Route 3, Greencastle, Ind.
LONGYEAR JOHN M., III, Peabody Museum, Harvard University, Cambridge 38, Mass.
LOPATIN, IVAN A., University of Southern California, Los Angeles, Calif.
LORD, WILLIAM W., Paris Tanning Co., South Paris, Me.
LOTHROP, S. K., 1061 Madison Avenue, New York, N.Y.
LOWIE, ROBERT H., University of California, Berkeley 4, Calif.
LOY, CLARK M., Loy Hardware Co., Effingham, Ill.
LUBBE, CHARLES KIMBALL, 217 Columbia Terrace, Peoria, Ill.

MCALLISTER, J. GILBERT, Department of Anthropology, University of Texas, Austin, Tex.
MCCANN, CATHERINE J., 251 S. 41st Street, Philadelphia 4, Pa.
MCCARY, B. C., Box 123, College of William and Mary, Williamsburg, Va.
MCCLELLAN, LT. (j.g.) C., 3746 W Street, N.W., Washington 7, D.C.
MCCOWN, B. E., 3779 Kettner Boulevard, San Diego 1, Calif.
MCCOWN, THEODORE D., Department of Anthropology, University of California, Berkeley 4, Calif.
MCGREGOR, JOHN C., Centennial Building, Illinois State Museum, Springfield, Ill.
MCILHENNY, E. A., Avery Island, La.
MCKERN, W. C., Public Museum, Milwaukee 3, Wis.
MCKINNY, J. J., De Witt, Mo.
MACCORD, MAJ. HOWARD A., Hq. 167th Engr. Combt. Bn., A.P.O., 403, c/o Postmaster, New York, N.Y.
MACGOWAN, EARNEST S., 4112 Colfax Avenue S., Minneapllis, Minn.
MACGOWAN, KENNETH, 5451 Marathon Street, Paramount Pictures, Hollywood 38, Calif.
MACHAFFIE, E. D., c/o State Publishing Co., Helena, Mont.
MACPHERSON, J. H., PhM. 2/c, c/o U. S. Naval Hospital, Navy 10, c/o Fleet P.O., San Francisco, Calif.
MADEIRA, PERCY C., JR., 100 S. Broad Street, Philadelphia 10, Pa.
MADSEN, HARRY, Carpenter, S. Dak.
MAIRS, O. L., Box 4, North Charleston, W. Va.
MALOUF, LT. CARLING, Separation Center, Fort Douglas, Utah
MANGUS, MRS. ELLA B., 640 Riverside Drive, New York, N.Y.
MARRS, LT. GARLAND JOHN, 472 Whitney Avenue, New Haven 11, Conn.
MARTIN, LESLIE A., 9 Chateau Apartments, Charleston, W. Va.
MARTIN, PAUL S., Chicago Natural History Museum, Roosevelt Rd. and Field Drive, Chicago 5, Ill.
MARYE, WILLIAM B., 207 E. Preston Street, Baltimore 2, Md.
MASON, J. ALDEN, University Museum, 33rd and Spruce Streets, Philadelphia 4, Pa.
MATSON, FREDERICK R., 916 New Holland Pike, Lancaster, Pa.
MAYR, THOMAS, Davidsonville, Md.
MERA, H. P., Laboratory of Anthropology, Santa Fe, N.M.
MERRILL, RICHARD K., 925 Greenwood Boulevard, Evanston, Ill.
MERRILL, ROBERT H., 1111 Jefferson S.E., Grand Rapids, Mich.
METCALF, GEORGE, 18 5th Avenue, Haskell, N.J.
MEYER, RICHARD K., 3940 Prospect, Peoria, Ill.
MILFORD, STANLEY J., 519½ Grant Avenue, Santa Fe, N. Mex.
MILLER, CARL F., 622 N. 10th Avenue, Tucson, Ariz.
MILLER, P. SCHUYLER, 302 S. Ten Broeck Street, Scotia, N.Y.
MINER, DR. KENNETH W., 2428 Central Avenue, Minneapolis 13, Minn.
MITCHELL, MRS. CHARLES M., 123 North Sixth Street, Monroe, La.
MORGAN, MRS. C. P., 127 Marlborough Road, West Hempstead, Long Island, N.Y.
MORGAN, FRED J., 1235 Lincoln Avenue, Pasadena 3, Calif.
MORGAN, RICHARD, Ohio State Museum, Columbus, Ohio
MORLEY, SYLVANUS G., Apartado Postal 385 Merida, Yucatan, Mexico
MORRIS, CARLYLE, 7005 Union Avenue, Cleveland 5, Ohio
MORRIS, EARL H., P.O. Box 500, Boulder, Colo.
MORRIS, HARRY C., c/o Clayton L. Howard, 289 N. Main Street, Middleboro, Mass.
MORSS, NOEL, 568 South Street, Needham, Mass.
MULL, R. G., Dighton, Kans.
MULLOY, LT. WILLIAM T., JR., 1139 Martin, Ann Arbor, Mich.
MURRA, JOHN V., Department of Anthropology, University of Chicago, Chicago, Ill.

NASH, CHARLES H., W.V. 53, Clinton Engineering Works, Oak Ridge, Tenn.
NASH, R. P., 1763 Sheridan Road, South Euclid, Ohio
NEUMANN GEORG K., Department of Zoology, Indiana University, Bloomington, Ind.
NEWCOMB, SIMON J., Palmer, Alaska
NEWTON, J. J., Fairforest Finishing Co., Spartanburg, S. Car.
NOBLE, BRUCE, 414 College Street, Waxahachie, Tex.
NOBLE, DR. THOMAS B., JR., 1008 Hume Mansur Building, Indianapolis, Ind.
NOE, DR. CHARLES F., Amana, Iowa
NOGUERA, EDUARDO, Direccion de Monu-

mentos Prehispanicos, Secretaria de Educacion Publica, Mexico, D.F., Mexico
NUSBAUM, JESSE L., Mesa Verde National Park, Mesa Verde, Colo.

O'LOUGHLIN, KATHLEEN, 129 Ontario Street, St. Catharines, Ontario, Canada
OLSON, RONALD L., Department of Anthropology, University of California, Berkeley 4, Calif.
ORCHARD, FREDERICK P., Peabody Museum, Harvard University, Cambridge 38, Mass.
ORR, ELLISON, Waukon, Iowa
ORR, PVT. KENNETH G., 36694691, Hq. Det. 60th Infantry Training Bn., Camp Wolters, Tex.
OSBORNE, DOUGLAS, 1301 Marquette Road, Albuquerque, N. Mex.
OSGOOD, CORNELIUS B., Peabody Museum, Yale University, New Haven, Conn.
OVER, W. H., University of South Dakota Museum, Vermillion, S. Dak.

PALMATARY, HELEN C., Apartment 20 Arrot Arms, Frankford, Philadelphia, Pa.
PARKER, ARTHUR C., Rochester Museum of Arts and Sciences, Rochester, N.Y.
PARKER, NORMAN S., 3200 Willoughby Tower, 8 S. Michigan Boulevard, Chicago, Ill.
PASCO, GEORGE, 943 Metomen Street, Ripon, Wis.
PAUL, DR. JOHN R., 49 Autumn Street, New Haven, Conn.
PAXTON, C. L., 207 Willow Street, Charleston 2, W. Va.
PAYNE, FRANK A., General Delivery, Aguanga, Calif.
PECK, STUART, 3214 Urban Avenue, Santa Monica, Calif.
PELL, S. H., Fort Ticonderoga Museum, Fort Ticonderoga, N.Y.
PENNYPACKER, SAMUEL W., II, Pennypackers Mills, Schwenksville, Pa.
PETIT, GEORGE, 2702 North 12th Street, Terre Haute, Ind.
PFIRMAN, KENNETH R., 347 East 17th Street, Erie, Pa.
PHELPS, GIRARD W., 399 Massachusetts Avenue, Trenton 9, N.J.
PHILLIPS, PHILIP, Peabody Museum, Harvard University, Cambridge 38, Mass.
POOLE, JAMES, JR., 248 Bassett Avenue, Lexington, Ky.
POTTER, HAROLD, c/o Humble Oil and Refining Co., Tyler, Tex.
PRATT, LESLIE, Box 105, Bakersfield, Calif.
PREECE, EDMUND F., 8313 16th Street, Silver Spring, Md.
PRETZAT, HERBERT, 14024 14th Avenue, Whitestone, N.Y.
PROFFITT, HARRY, P.O. Box 147, Hastings, Nebr.
PROSKOURIAKOFF, TATIANA, 10 Frisbie Place, Cambridge 38, Mass.
PROVINSE, S. M., Red Lodge, Mont.

QUIMBY, GEORGE I., JR., Chicago Natural History Museum, Roosevelt Rd. and Field Drive, Chicago 5, Ill.

RACER, DR. F. H., Box 457, Lomita, Calif.
RANDS, ROBERT L., 401 Fifth Street, N.E., Washington 2, D.C.
RASMUSSON, R. D., 417 S. Vine Street, North North Platte, Nebr.
RAY, J. HENRY, Yamparika and Fannin Streets, Vernon, Tex.
REED, PVT. ERIK K., P.O. Box 461, Santa Fe, N. Mex.
REEVES, COL. DACHE M., 0-10462, Hq. First Tactical Air Force, A.P.O. 374, c/o Postmaster, New York
RETZEK, REV. HENRY, c/o St. Alexius Church, West Union, Minn.
REYNOLDS, MARY I., 1140 Jefferson Building, Peoria 2, Ill.
RICHARDS, DARREL J., 6584 Balfour, Allen Park, Mich.
RICHARDSON, FRANCIS B., 10 Frisbie Place, Cambridge 38, Mass.
RICKETSON, OLIVER G., 9 West Cedar St., Boston 8, Mass.
RIGHTS, REV. DOUGLAS L., Box 133 Salem Station, Winston-Salem, N. Car.
RITCHIE, WILLIAM A., Rochester Museum of Arts and Sciences, Rochester, N.Y.
RITTER, HARTIEN S., 826 Henry Street, Alton, Ill.
ROBBINS, MAURICE, 25 Payson Street, Attleboro, Mass.
ROBERTS, FRANK H. H., JR., Bureau of American Ethnology, Smithsonian Institution, Washington 25, D.C.
ROBERTS, HELEN H., Tryon, N. Car.
ROBERTS, TRAVIS, Marathon, Tex.
ROBERTS, WATKINS W., 40 Elm Street, Methuen, Mass.
ROGERS, SPENCER L., 920 Martinez Street, San Diego 6, Calif.
ROOP, CAPT. W. P., Anchorage Farm, Sewell, N.J.
ROOT, W. C., 226 Maine Street, Brunswick, Me.
ROUSE, IRVING, Peabody Museum, Yale University, New Haven, Conn.
ROWE, PVT. JOHN H., 104 Crofton Road, Waban 68, Mass.
ROWE, PAUL, Route 1, Glenwood, Iowa
ROYS, LAWRENCE, 825 27th Street, Moline, Ill.
ROYS, RALPH, 1512 Olin Place, Seattle 2, Wash.
RUPPERT, KARL, 10 Frisbie Place, Cambridge 38, Mass.
RUSHTON, MRS. L. C., Box 235, Mercedes, Tex.
RUSTE, L. GERHARD, Montrose, S. Dak.

SATTERTHWAITE, LINTON, JR., University Museum, 33rd and Spruce Streets, Philadelphia 4, Pa.
SAYLES, E. B., Arizona State Museum, Tucson, Ariz.
SCHAEFER, VINCENT, Box 31, R.D. 3, Schermerhorn Road, Schenectady, N.Y.
SCHAEFER, C. E., 10 Front Street, Shiremanstown, Pa.
SCHMIDT, PAUL H., Suite 703, Old National Bank Building, Evansville, Ind.
SCHMITT, KARL, JR., 6100 Greenwood Avenue, Chicago 37, Ill.
SCHNEIDER, ROBERT, 1991 Ferdinand, Detroit 9, Mich.
SCHOENBECK, GEORGE, 821 Heading Avenue, Peoria 5, Ill.
SCHREINER, M. C., 28 12 119th Street, College Point, N.Y.
SCHROEDER, ALBERT H., 464 Heights Road, Ridgewood, N.J.
SCHULTZ, C. BERTRAND, University of Nebraska State Museum, Lincoln 8, Nebr.
SCHUMACHER, PAUL, 3983 Innverness Drive, Houston, Tex.
SCHWAB, ROBERT CHARLES, 955 Revere Drive, Hillside 5, N.J.
SCOTT, DONALD, Peabody Museum, Harvard University, Cambridge 38, Mass.
SETZLER, F. M., U. S. National Museum, Washington 25, D.C.
SHAFLIN, JUDSON T., NATS Pacific, c/o Fleet Post Office, San Francisco, Calif.
SHEPARD, ANNA O., 751 11th Street, Boulder, Colo.
SHERMAN, CHARLES F., R.F.D. 1, Plymouth, Mass.
SHERMAN, THEODORE H., 29 West Street, Fair Haven, Vt.
SHETRONE, H. C., Ohio State Museum, Columbus, Ohio
SHIPEK, MRS. FLORENCE C., 4223 Latona, Seattle, Wash.
SHIPPEE, J. M., R.F.D., 10, North Kansas City, Mo.

SHURROCKS, ALFRED R., 16 Vestal Street, Nantucket, Mass.
SHURROCKS, MRS. ALICE A., 16 Vestal Street, Nantucket, Mass.
SINGER, WALTER, 58 26th Street, Wellsburg W. Va.
SKARLAND, PVT. IVAR, Headquarters Co., Alaska Defense Command, A.P.O. 942, c/o Postmaster, Seattle, Wash.
SLEIGHT, LT. FREDERICK W., Disbursing Officer, Naval Supply Depot, Clearfield, Utah.
SMAIL, WILLIAM, R.R., Loogootee, Ill.
SMITH, A. LEDYARD, 2511 N. Farwell Avenue, Milwaukee 11, Wis.
SMITH, BENJAMIN L., 64 Sudbury Road, Concord, Mass.
SMITH, C. E., JR., 2306 Durant Avenue, Berkeley, Calif.
SMITH, SGT. CARLYLE S., 114 Susquehanna Avenue, Great Neck, N.Y.
SMITH, ELMER R., 277 East 6th S., Salt Lake City 2, Utah
SMITH, HALE, 11233 South Western Avenue, Chicago 43, Ill.
SMITH, MRS. MILDRED C., 502 Cleveland Place, Boulder, Colo.
SMITH, ROBERT ELIOT, Carnegie Institution, 9a Avenida Norte No. 54, Guatemala, C.A.
SMITH, VICTOR J., Teacher Training Department, Armored Force School, Fort Knox, Ky.
SMITH, WATSON, 18 E. Fourth Street, Cincinnati, Ohio
SMITH, WILLIAM NEIL, II, Box 4784, Tucson, Ariz.
SNOW, CHARLES E., 1851 S. Limestone Street, Lexington 33, Ky.
SODAY, FRANK J., 2016 Cherokee Street, Baton Rouge, La.
SOLECKI, RALPH, 6141 56th Avenue, Maspeth, N.Y.
SPAULDING, ALBERT C., School of Forestry, Montana State University, Missoula, Mont.
SPECK, FRANK G., University of Pennsylvania, Philadelphia 4, Pa.
SPEIGHT, GLENN, West Branch, Iowa
SPENCER, ROBERT F., c/o Department of Anthropology, University of California, Berkeley 4, Calif.
SPIER, LESLIE, P.O. Box 880, Santa Cruz, Calif.
SPINDEN, H. J., Brooklyn Museum, Brooklyn, N.Y.
SPITZER, CARL B., 430 Spitzer Bldg., Toledo 4, Ohio
SPOEHR, LT. ALEXANDER, 270 Birch Road, Winnetka, Ill.
SPRANG, MRS. SUSAN G., Ohio State Museum, Columbus, Ohio
STACKELBERG, THE BARON, 1919 37 Street, N.W. Washington 7, D.C.
STEPHENS, B. W., 815 Broadway, Quincy, Ill.
STEPHENSON, ROBERT L., Lakeview, Ore.
STEWARD, JULIAN H., Bureau of American Ethnology, Smithsonian Institution, Washington 5, D.C.
STEWART, KENNETH M., 353 Geary Street, San Francisco, Calif.
STEWART, OMER C., 1639 Wisconsin Avenue, N.W., Washington 7, D.C.
STILES, N. L., Cherokee, Iowa
STIRLING, M. W., Bureau of American Ethnology, Smithsonian Institution, Washington 25, D.C.
STONE, CLAUDE U., 210 W. Armstrong, Peoria, Ill.
STONE, MRS. DORIS Z., Apartado 1309, San Jose, Costa Rica, C.A.
STONE, R. J., Chewelah, Wash.
STONER, REV. VICTOR R., Greenway Station, Tucson, Ariz.
STRAIGHT, DAVID M., Harwood Road, East Greenwich, R.I.
STRANDT, HERMAN F., 1025 E. Broadway, Anaheim, Calif.
STROEBEL, RALPH W., 117 Storch Street, Saginaw, Mich.
STRONG, WILLIAM DUNCAN, Department of Anthropology, Columbia University, New York 27, N.Y.
STUART, BRADLEY R., Moapa, Nev.
STUDER, FLOYD V., 637 Amarillo Building, Amarillo, Tex.
STURDY, HENRY FRANCIS, 85 Shipwright Street, Annapolis, Md.
SWANTON, JOHN R., Smithsonian Institution, Washington 25, D.C.
SWART, JOHN A., R.F.D. 3, Amsterdam, N.Y.

TATUM, MIDSHIPMAN ROBERT M., Apartment 10, 1215 N. Nevada, Colorado Springs, Colo.
TAYLOR, WALTER W., Jr., c/o William P. Eno, Saugatuck, Conn.
THOMAS, CHESTER A., Box 1321, Bandelier National Monument, Santa Fe, N. Mex.
THOMAS, J. E., Chickering Lane, R. 5, Nashville 5, Tenn.
THOMPSON, J. ERIC S., Harvard, Mass.
TILDEN, ROBERT H., 6 Turner Road, Scituate, Mass.
TIPLING, L. B., Power Black Annex, Helena, Mont.
TITTERINGTON, DR. P. F., 212 Metropolitan Building, St. Louis, Mo.
TOMPKINS, P. W., P.O. Box 411, San Anselmo, Marin County, Calif.
TORREY, HOWARD, 247 Summer Avenue, Reading, Mass.
TOULOUSE, MRS. JOSEPH H., JR., 628 Gomez Road, Santa Fe, N. Mex.
TOWER, DR. DONALD B., University of Minnesota Hospitals, Minneapolis 14, Minn.
TOWLE, MRS. MARGARET A., 3 Concord Avenue, Cambridge, Mass.
TOZZER, ALFRED M., 7 Bryant Street, Cambridge 38, Mass.
TREGANZA, ADAN E., 2729 Regent Street, Berkeley, Calif.
TROWBRIDGE, H. M., Route 1, Bethel, Kans.
TUTHILL, CARR, Box 27, Clinton, Wash.
TYZZER, DR. ERNEST E., Harvard Medical School, 25 Shattuck Street, Boston, Mass.

UNDERWOOD, JOHN E., Box 155, Dubois, Wyo.

*VALCARCEL, LUIS, Museo Nacional Del Peru, Lima, Peru
VALLADARES, MRS. J., Museu Do Estado, Salvador Bahia, Brasil, S.A.
VAN STEN, INA, Florida State College for Women, Tallahassee, Fla.
VIETZEN, RAYMOND C., 227 Temple Court, Elyria, Ohio
VOEGELIN, C. F., Social Science Building, Indiana University, Bloomington, Ind.
VON SCHLICHTEN, HAROLD C., 2705 Eden Avenue, Cincinnati, Ohio

WAGNER, RICHARD K., 1922 Oakdale Avenue Chicago 13, Ill.
WALKER, EDWIN F., The Southwest Museum, Highland Park, Los Angeles 42, Calif.
WALKER, WINSLOW M., 4553 Latona Ave. c/o Mrs. M. C. Roff, Seattle 5, Wash.
WALSER, P. H., 1102 North 1st Street, Temple, Tex.
WARD, LAURISTON, Peabody Museum, Harvard University, Cambridge 38, Mass.
WARDLE, H. NEWELL, Sharon Hill, Pa.
WARING, LT. ANTONIO J., JR., 3415 38th St., N.W. Washington 16, D.C.
WASHBURN, EDWARD, Hotel Jefferson, Iowa City, Iowa
WATSON, M. M., Valdez, Colo.
WATT, FRANK H., Box 1176, Waco, Tex.
WEBB, DR. C. H., 1560 Line Avenue, Shreveport, La.

WEBB, WILLIAM S., University of Kentucky, Lexington, Ky.
WECKLER, JOE, JR., Dept. of Anthropology, University of Southern California, Los Angeles 7, Calif.
WEDEL, WALDO R., U. S. National Museum, Washington 25, D.C.
WEER, PAUL W., 5650 N. Meridian Street, Indianapolis 8, Ind.
WEIANT, C. W., 55 West 42nd Street, New York, N.Y.
WEIR, ALASTAIR M., 9825 Grand River Avenue, Detroit 4, Mich.
WELLMAN, PAUL I, 11365 Denair Street, Los Angeles 24, Calif.
WESCOTT, W. B., Springdale Avenue, Dover, Mass
WHALEY, J. KIRKWOOD 6438 N. Richard Street, Milwaukee, Wis.
WHEAT, JOSEPH B., Van Horn Tex.
WHEELER, LT. S. M., c/o Spruse Reitz Co., Portola, Calif.
WHITE, MARIAN E., Gasport, N.Y.
WHITEFORD, ANDREW H., Logan Museum, Beloit College, Beloit, Wis.
WILFORD, LLOYD A., Department of Anthropology, University of Minnesota, Minneapolis, Minn.
WILL, GEORGE F., 322 Fourth, Bismarck, N. Dak.
WILLEY, GORDON R., Bureau of American Ethnology, Smithsonian Institution, Washington 25, D.C.
WISSLER, CLARK, 15 W. 77th Street, New York, N.Y.
WITHERS, ARNOLD M., 1825 Grand Avenue, Pueblo, Colo.
WITTE, A. H., Henrietta, Tex.
WOGAN, S. F., Wogan Auto Electric, Montevideo, Minn.
WOODBURY, RICHARD B., 1801 Hoban Road, N.W., Washington 7, D.C.
WRAY, CHARLES F., West Rush, N.Y.
WRAY, DONALD E., 5526 Ellis Avenue, Chicago 37, Ill.
WRIGHT, ROSS PIER, 235 W. 6th Street, Erie, Pa.
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